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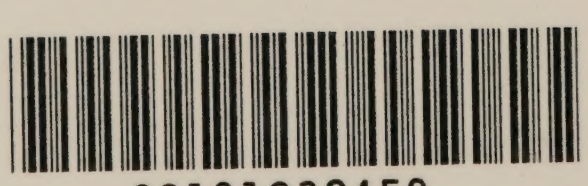
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THE ASCLEPIAD.

A Book of Original Research and Observation

IN THE

SCIENCE, ART, AND LITERATURE OF MEDICINE, PREVENTIVE AND CURATIVE.

SECOND SERIES.

1891.

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To

S. WEIR MITCHELL, M.D.,

OF PHILADELPHIA, U.S.A.,

PHYSICIAN, POET, SCHOLAR,

IN RECOGNITION OF HIS MANY ORIGINAL AND FRUITFUL

CONTRIBUTIONS TO PHILOSOPHICAL MEDICINE,

AND IN REMEMBRANCE OF

OVER THIRTY YEARS OF FRIENDLY COMMUNION,

THIS,

THE EIGHTH VOLUME OF THE "ASCLEPIAD,"

IS AFFECTIONATELY INSCRIBED

BY

THE AUTHOR.

PREFACE

TO THE EIGHTH VOLUME OF THE "ASCLEPIAD."

IN this volume of the ASCLEPIAD, in addition to other contributions, three special essays have been introduced—namely, on *Peroxide of Hydrogen; National Main Drainage;* and *The Cerebro-spinal Axis as a Thermal Centre and Water Power.* I had intended, in commencing the first of these essays, to complete it in two parts; but such was the interest taken by readers in it, I have carried it through the four parts, and have left, I trust, a fair practical representation of what may be said, up to the present time, on a very important therapeutical topic. The second essay, on National Main Drainage, has become matter of so much public interest, outside the lines of physic, that a separate publication is called for, and some hope is breaking that the scheme proposed may take, before long, a practical form. The last essay, on the Cerebro-spinal Axis, is that on which my mind has been most occupied during the year. It is the principal effort of the present volume, and calls for continuous and anxious labour in the future.

I am much honoured by the many requests sent me to extend the review department of the ASCLEPIAD. The fact indicates a degree of confidence in my work which is as gratifying as it is reassuring. It encourages me to commence the first part of next volume (No. 33) with an

addition of twelve pages, so as to enable me to carry out the desire expressed without curtailing the other sections of the work.

I take the opportunity, in closing this preface, of stating, once again, that the term "Editor," daily addressed to me, in connection with the ASCLEPIAD is incorrect. It has become necessary to renew this explanation not only for saving the time and labour of unnecessary correspondence, but also for preventing mistakes as to authorship which are not infrequently and incorrectly made. The ASCLEPIAD is a *book written from first to last by myself alone*, and will continue so to be.

BENJAMIN WARD RICHARDSON.

25, MANCHESTER SQUARE, LONDON, W.,
November 15*th*, 1891.

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ON PEROXIDE OF HYDROGEN: A PHYSICAL-MEDICAL RESEARCH.

CONTINUED FROM A RESEARCH COMMENCED IN THE YEAR 1856.

PART I.—PHYSICAL AND PHYSIOLOGICAL.

IN the year 1856 I commenced the study of peroxide of hydrogen with the view of introducing it into medicine as an addition to our treasury of remedies. The history of this singular chemical body had at that time almost become forgotten, and no one, as far as I could learn, had ever taken it up in respect to medical research, although, as I afterwards ascertained, a seat of learning at Haarlem did for some years offer a prize for such a research, which prize was withdrawn not long before my work was entered upon, simply because there had never been a competitor. The peroxide, when I commenced my investigations, was a chemical curiosity, so rare I could not obtain it in quantity, and was, therefore, obliged to set up the necessary apparatus for its manufacture in my own laboratory. After I found that there was an opening for the peroxide in medicine, I asked Mr. Lloyd Bullock to manufacture the solution on a sufficiently large scale, and he made two or

three Winchester quarts of it; but as it did not fall in with his occupations, at that time, to continue the manufacture, it was undertaken by Mr. Robbins of Oxford Street, who devoted the utmost care in carrying out all my wishes, and who has continued to manufacture the purest specimens of it for medical purposes up to the present time. Peroxide of hydrogen was discovered and described in the year 1818 by the illustrious French chemist, Baron Thenard.

ORIGIN OF THE RESEARCH.

It may gratify a new generation of members of the medical fraternity if I very briefly relate what led me to use this peroxide in the first instance. It was in this way: I was working at ozone, and had been studying, with the late Dr. Moffat of Hawarden, the changes of disease in what he called "ozone periods." I made some hydrogen peroxide, and found that the oxygen condensed in it coloured ozone test-papers of starch and potassium iodide, a fact showing that this oxygen was more active than the oxygen of common air. It then occurred to me that the peroxide might be of service as a medicine if its action on the animal body and on the tissues were well investigated and defined. In those days we were inclined to believe that diabetes was due to the deficient oxidation of the tissues,—a theory which an original observation of mine, namely, that dogs could be made to eliminate urine containing glucose, by subjecting them to an atmosphere of carbonic oxide, seemed to confirm. As a result of this line of thought I made an original investigation, the results of which I reported at length in a paper read to the Medical Society of London in 1860, describing the chemical, physiological, and therapeutic action of the substance, and explaining: (*a*) That the

peroxide quickens oxidation, increases secretion, arterializes blood, and hastens the decomposition of decomposing animal tissues; (*b*) That in combination with blood it restores the power of contraction to muscles recently dead, and yet calms muscular irritability; (*c*) That it reduces muscular rigidity both before and after death; (*d*) That in instances of poisoning by narcotics and by the alkaloids it might, with advantage, be introduced into the blood by transfusion or injection; (*e*) That in tetanus it affords a most rational treatment, since tetanic rigidity is relaxed by its presence in muscle; (*f*) That in typhus, in which death occurs from what I designated *asphyxia commencing in the blood*, it might be employed with good effect as an oxygenator; (*g*) That it ought to prove useful both as an internal and external remedy in the treatment of cancer. I also described, in this paper, the best form of the peroxide for medical use, suggesting ten-volume strength as the most certain and convenient for ordinary administration.

At the meeting of the same Society on March 3rd, 1862, I made a second report on the medicinal use of the peroxide. I had by this time used it in two hundred and twenty-three cases of disease, including phthisis, diabetes, anæmia, sub-acute and chronic rheumatism, strumous enlargement of the cervical glands, mesenteric disease, pertussis, chronic bronchitis, chronic laryngitis, mitral disease, and dyspepsia. In epitome of results I drew the conclusions: (*a*) That in diabetes the peroxide reduced the specific gravity of the urine, whilst it rather increased the quantity; (*b*) That in chronic and sub-acute rheumatism it afforded relief; (*c*) That in valvular disease of the heart with pulmonary congestion it gave relief to the dyspnœa; (*d*) That in mesenteric disease and in jaundice it caused an improvement in the digestion; (*e*) That in pertussis its effect for good

was very remarkable, since it cut short the paroxysms of cough, and seemed decidedly to shorten the period of the disease; (*f*) That in chronic bronchitis it lessened the dyspnœa, and rendered the expectorated matter less tenacious; (*g*) That in chronic laryngitis it gave pain on being swallowed, and did not appear to be useful; (*h*) That in anæmia it did not of itself render any service, but favoured the good effect of iron; (*i*) That in the first stage of phthisis it caused improvement in the digestion, and in the later stages gave unquestionable and even wonderful relief to the breathlessness and oppression, acting, in fact, like an opiate without narcotism, and assisting oxidation.

In the discussion which followed upon the reading of this paper I was warmly supported in several points by Drs. Septimus Gibbon, Symes Thompson, and Gibb, all of whom had been prescribing the peroxide on the suggestions made in my previous paper of 1860. Dr. Gibb bore special testimony to its value in affording relief during the last stage of phthisis, for which I had recommended it in the case of a member of his own family. But the most important new observation I had to communicate to the Society in 1862 was that in free and frequently repeated doses the peroxide could be made to produce a modified salivation, a fact which led to two suggestions: firstly, that in the use of mercurial and iodide preparations it was the chlorine or iodine in them which caused the ptyalism; secondly, that the peroxide would be a good substitute for mercury and the iodides in the treatment of syphilis.

It will be seen from these brief preliminary notes that my research had made a good start in 1862. From that time until now the work has never left my hands, and although it has been followed up intermittently, obstructed,

now and then, for months by other labours, it has so accumulated that I have matter for a decent volume before me which I hope soon to publish. From the pages of this intended treatise let me collect a few of the more important points for the present purpose.

It will be most convenient to divide what I have to relate into four heads: (1) Physical; (2) Physiological; (3) Practical or Therapeutical; (4) Pharmacological. But before entering on either point it will be necessary to devote a few moments to a description of some of the special qualities of the peroxide solution; otherwise I shall be talking an unknown language to those who are not conversant with certain primary facts, themselves unique in the history of chemical substances.

PHYSICAL.

Hydrogen peroxide must be looked upon as water containing, according to its strength, so many atmospheres of ozonized oxygen. It is an ozonized oxygen atmosphere in solution. It is not, however, a mere mixture, but a peculiar chemical compound. The oxygen can be made to accumulate, volume by volume, until the volume of water, say as much as would fill a pint measure, can rise to ten, twenty, thirty, and some say even a hundred and twenty pints of oxygen, before complete saturation is reached and a volatile body is formed. We hold, therefore, in a specimen of the peroxide, condensed oxygen combined either with the hydrogen of the water, or with the oxygen of the water, or with the elements HO acting as a radical. There is here not much difference, at first sight, from what is common in combinations where there is accumulation of one element on another; as, for example, in the combination of carbon with one equivalent of oxygen in carbon monoxide, and

carbon with two equivalents of oxygen in carbon dioxide. But now comes a distinction. The combination of the added oxygen in hydrogen peroxide is stable in the presence of some substances, unstable and easily evolved in the presence of others. Some substances, inorganic or organic, when added to the solution are neutral; other substances, inorganic or organic, evolve the oxygen and are themselves unchanged; a third kind evolve the oxygen, and with that some of their own contained oxygen; a fourth kind absorb the oxygen into themselves.

We can illustrate this by experiment. Let seven flasks be equally charged with a ten-volume solution of the peroxide. To flask *one* add a portion of peroxide of iron: the effect is neutral. To flask *two* add peroxide of platinum: there is brisk action, so that one can light a taper from the oxygen thrown off, but the platinum remains unchanged. To flask *three* add permanganate: there is very brisk action, and with the action evolution of oxygen from the manganese as well as from the solution. To flask *four* add arsenious acid: there is absorption of the oxygen by the arsenious compound. These are inorganic bodies; let us compare them with organic. Into flask *five* put albumen: the action is neutral. Into flask *six* put fibrine: there is brisk evolution. Into flask *seven* put red blood corpuscles: there is absorption. The analogy is perfect with one exception. I cannot find an organic substance that liberates the oxygen and at the same moment yields up its own oxygen. There may be such a body, but I have not yet seen it. Thenard, in a general way, had a wonderful knowledge of these distinctions, but he did not live to follow them completely out or to apply them. They are all important to our medicinal labours, in pointing the way to success

by the experimental instead of the empirical mode of questioning nature.

Of the solution itself, I may say that the more I have experimented with it the stronger have become my doubts as to its being a true chemical combination of oxygen with hydrogen, like that which obtains between oxygen and hydrogen in water. The taste of the solution is that of ozone, and is very persistent. The combination is very feeble, and very easily disturbed; the combined oxygen is extremely active, in fact like ozone whilst in combination. I am therefore inclined to think that it is ozone chemically combined with water, and I would be inclined to call it aqueous ozone, or ozonized water.

PHYSIOLOGICAL.

In my researches I took up, one by one, the natural tissues of the body, in order to test them in the presence of the peroxide solution. In the end, for I have not space for details, the general fact which seemed fairly to have been elicited was that all structures of the body which have not become vitalized structures—albumen, for instance—have no power to displace the oxygen; neither have structures which have played their part and are actually dead, like nail and squamous cuticle, elastic tissue and mucus; whilst every structure that has become animalized and is passing through the stage of vital action does possess the power of displacement. Some substances, like red blood corpuscles, absorb the oxygen, as we have seen.

To put the matter now in the briefest possible form, living cell structure of every kind presented to the peroxide solution, if it does not absorb, liberates the oxygen, and seems to distinguish by that phenomenon organic structure that has become cellular organic and vital from

that which is not yet cellular or organic, and from that which has passed from the organic and vitalized state to the inorganic and dead. Amongst organic substances those which approach nearest to what we, in our present state of knowledge, call "vital" show most activity of action, and those which have not lived, or which, having lived, are being cast off as dead, show least. To this latter there are one or two exceptions; the carbonic acid exhaled by the breath being the most notable, if we accept that gas as an organic substance.

Step by step in my progress in these researches I was led, almost imperceptibly, to another on which I have devoted much time, every enquiry being by the nature of it a slow process. The question opened was as follows: What effect will the oxygen liberated from a solution of the peroxide have on the organic substance that liberated it? This question also soon included two more, namely, How far will the effect be moderated (*a*) by pressure; (*b*) by temperature; (*c*) by pressure and temperature in combination?

ACTION ON NORMAL SURFACES.

The action in the free state was very clear. Placed simply on open surfaces, so as to commingle with organic substances that will liberate it, the peroxide, by its liberated oxygen, produces decomposition, in turn, of the organic material. The process of the decomposition of the organic material is much expedited by an elevation of temperature, and this again is increased by pressure.

In all these researches I kept the following leading principle in mind. This substance, peroxide of hydrogen, is another or second atmosphere. It is a condensed liquid oxygen, part of the oxygen of which is easily liberated by mere contact with some substances belonging

to the body. The breath liberates it; ergo, I might use my breath as a liberator. Here was ground for a new experiment. I used a mask after the manner of the one first constructed for anæsthesia. The mask had two openings into it. With one of these I connected a tube running to the long inlet tube of a Wolff's bottle; the other was attached to the exit tube of the same bottle, with an elastic bag intervening. Then I put on the mask so as to exclude myself entirely from the common air, and breathed forcibly through the solution. My breath caused evolution of oxygen, which was caught in the intercepting bag from which I breathed the gas and lived on it. By this means, even without removing the carbonic acid of the expired air, I could maintain life double the time it would be sustained in a negative medium, under water, for example. In this experiment I somewhat anticipated Fleuss in his diving apparatus, but followed St. Simon Sicard, who with condensed oxygen had done the same thing in his diving apparatus, although I did not know the fact, in 1854.

I next tried if the peroxide solution would serve for respiration, like oxygen, by immersing fishes in water from which the ordinary oxygen had been expelled, and to which neutral peroxide solution had been added. From these experiments I deduced that the peroxide had the power of sustaining the life of fishes, but not so perfectly as the oxygen present in common water.

In another line of research I sought to ascertain what would be the effect of introducing the solution into the alimentary canal, and into other cavities lined with mucous membrane and protected by mucous secretion. The result was, that if the cavities contained no foreign substance capable of liberating the oxygen, there was no action. Neither the bladder nor the uterus evolved the

oxygen if they were in the natural state, but if they contained pus, or if their surfaces were broken in continuity, so that cellular tissue was exposed, then the solution, coming in contact with the purulent substance, or the cellular structure, was directly freed of its extra oxygen. If there were no cause leading to the liberation of the oxygen while the fluid was in the mucous cavity, the fluid was either ejected, or was absorbed into the circulation. If it were absorbed on entering into the circulation the oxygen was set free and was taken up by the corpuscles unless it were in too large a quantity for immediate absorption, when it would accumulate in the chambers of the circulation, and destroy, as air does, after air is injected into a vein.

From the mucous surfaces I moved to the serous by injecting the fluid through the hypodermic needle into the closed peritoneal cavity. The animals operated upon were under chloroform, and it was observed that so soon as the fluid reached the peritoneal cavity the breathing became much quickened, but there was no sign whatever of distension or of escape of the gas. Very soon afterwards the animals, still unconscious, died, and on opening their bodies immediately after death the cause of death was discovered. The solution injected had been absorbed into the inferior vena cava, and the blood in that vessel, in the right cavities of the heart, and in the pulmonary artery, was full of oxygen gas in the free state, by which the course of the blood over the lung was mechanically prevented.

In one of these experimental enquiries in 1868 I met with an experience I shall never forget. The animal under observation was a large strong cat, which had been subjected to the lethal action of chloroform. I allowed the animal to sleep to death, and after it

was dead it occurred to me to test the action of the peroxide solution in the peritoneum; first, to see if the fluid would excite the diaphragm to action; and, secondly, to observe if it would be absorbed after death. I injected a fluid ounce of the solution of ten-volume strength into the cavity through the hypodermic needle. To my surprise the breathing, which had ceased for several minutes, recommenced in full vigour, and in regular rhythm, but there was no return motion of the heart. Thereupon I opened the chest, and found the veins and right side of the heart filled with blood charged with loose oxygen. For a time I followed up this experiment under the hope that I might discover a simple application of the process for restoring life after death from chloroform in the human subject. At this time, however, my experimental researches had to cease. I shall revert to them as repeated at a later stage.

From the act of injecting the mucous and serous cavities with the peroxide solution, I proceeded to inject the solution, subcutaneously, into the cellular tissue. The effects were well marked; the fluid as soon as it was introduced into the cellular tissue gave up its oxygen. The gas diffused readily, producing some temporary emphysema, and was then absorbed into the circulation, giving rise to an elevation of temperature, and changing the colour of the venous blood towards the arterial hue. No important local mischief accrued from the operation.

I made a bolder experiment. To an animal deep under chloroform I introduced the peroxide solution, directly, by injecting it through a fine needle into the lung structure itself, puncturing through an intercostal space. This caused an oxygen diffusion into the lung, during which the animal lived, in one instance for five minutes, with the respiration entirely cut off.

In an experiment on the muscles of an animal under chloroform I repeated what I had already done for removing muscular rigidity, but in a different way. Ammonia injected into a living muscle excites contraction tetanic in character. When this had been produced, the peroxide solution, warmed to the temperature of 100° Fahr., was injected slowly, with the effect of producing relaxation. In a further trial the muscles of a narcotized animal were brought into contraction by a Faradic current, and in this state the muscles were injected with the solution at blood temperatures, with the effect of overcoming the resistance produced by the current, and of relaxing the muscles until the tension was increased.

ACTION ON ABNORMAL SURFACES.

From the study of normal structures I passed to the study of some which are abnormal, tissues changed by disease, and compared the normal with the abnormal. I compared, for instance, ordinary mucous secretion with purulent secretion, and found that whilst the mucous secretion, in its simple form and freed of carbonic acid, was absolutely negative, purulent secretion, so long as it was free of decomposition, acted with great effect. I tested fresh translucent lymph by the side of pus. I took fresh clear lymph from a vaccine vesicle. The lymph caused no liberation of oxygen. When the vesicle had become purulent, I tested again, and found that the purulent matter liberated the gas actively. I let some purulent matter dry, in the usual way, on an ivory point, and saw that it still retained its activity because it had not decomposed. I let another portion, from the same source, pass into decomposition and then tested it: it failed in producing any effect.

Purulent matter possesses, then, strongly, the power of

liberating oxygen from the peroxide, and probably the white corpuscles of the blood do the same. It may also be that the minute organisms called bacteria have the like power. In all cases the starting of the process is one of infinite subdivision of particular kinds of matter having a common property, and we may expect that in due time the common mode of their action as reducers of such compound bodies as peroxide of hydrogen, will be discovered. This is one of the most important problems for solution in the whole range of medical science and art, because every condition of disease in its acute form, involving organic change of structure, depends primarily on the decomposition of oxides of the tissues, as I hope to demonstrate should my life be spared to complete the enquiries I am systematically carrying out.

In testing the action of the peroxide on natural organic structures which liberate oxygen from it, I observed, as related above, that the fluid oxygen causes, in some instances, decomposition of the organic matter. The same fact was observed with abnormal organic material like pus. When pus is placed for observation under the microscope, mixed with the neutral peroxide solution, the phenomena are most interesting. The pus corpuscles are, for a time, driven about as if they were alive. They move in all directions, assume ovoid shapes as they squeeze through masses that may obstruct their course, and after many variations of form and movement come to a standstill, like amorphous matter, dead, so to speak, and entirely disorganised. This effect of the peroxide in destroying pus cells led me very early in these researches to use the solution for the treatment of suppurating surfaces, and with great success, as will appear in a future chapter.

COLD AND MORTALITY.

DURING the seven weeks of extreme atmospheric cold in which the last year ended and with which the present year opened, every one has been startled by the mortality that has prevailed amongst the enfeebled and aged population. Friends have been swept away in a manner most painful to recall, under the influence of an external agency, as natural as it is fatal in its course, and over which science, as yet, holds the most limited control.

In the presence of these facts questions occur to the mind which have the most practical bearing. Why should a community wake up one day with catarrh or with the back of the throat unduly red and the tonsils large? Why, in a particular village or town, shall the medical men be summoned on some particular day to a number of places to visit children with croup? What is the reason that cases of sudden death, by so-called "apoplexy," crowd together into a few hours? Why, in a given day or week, are shoals of the aged swept away, while the young live as before? These are questions which curative and preventive medicine have not yet mastered as might be desired. Curative medicine, at the name of them, too often stands abashed, if her interpreter be honest; and preventive medicine says, if her interpreter be honest, "The questions wait as yet for full interpretation."

Still, we are not altogether ignorant: some circumstances appear to be followed by effects so definite, that we may almost consider we have before us, in true position, cause and effect. Let us look at this position in reference to *the simple influence of temperature on the value of life.*

If we observe the fluctuation of the thermometer by the side of the mortality of the nation at large, no calculable relationship seems, at first sight, to be traceable between the one and the other. But if, in connection with the mortality, care be taken to isolate cases, and to divide them into groups according to the ages of those who die, a singular and significant series of facts follow, which show that after a given age a sudden decline of the temperature influences mortality by what may be considered a definite law. The law is, that variations of temperature exert no marked influence on the mortality of the population under the age of thirty years; but after the age of thirty is reached, a fall of temperature, sufficient to cause an increased number of deaths, acts in a regular manner, as it may be said, in waves or lines of intensity, according to the ages of the people. If we make these lines nine years long, we discover that they double in effect at each successive point. Thus, if the fall in the temperature be sufficient to increase the mortality at the rate of one person of the age of thirty, the increase will run as follows: 1 death at 30 years of age will become 2 deaths at 39 years of age, 4 at 48 years, 8 at 57 years, 16 at 66 years, 32 at 75 years, and 64 at 84 years.

In these calculations nothing seems to be wanting that should render them trustworthy: they resulted from inquiries conducted on the largest scale; they were computed by one of our greatest authorities in vital statistics,

the late Dr. William Farr, and they accord with what we gather from common daily observation. They supply, in a word, the scientific details and refinements of a rough estimate founded on universal experience, and they lead us to think very gravely on many subjects which may not have occurred to us before, and which are as curious as they are important.

We often hear persons who know little about vital phenomena, by which term I mean nothing mysterious, but simply the physics embraced in those phenomena which we connect with form and motion under the term life, harping on the one string, that man knows nothing of the laws of life and death. But what an answer to such presumption do the facts rendered above supply. Life and death are here reduced, on given conditions, to reasonings as clear and positive as are the reasonings on the development of heat by the combustion of fuel. It is not necessary for the vital philosopher to go out into the towns and villages to take a new census of deaths to enable him to give us his readings of the general mortality under the conditions specified. He may sit in his cabinet, and, as he reads his thermometer day by day, predict results. There is a fall of temperature that shall be known by experience to be sufficiently deep and prolonged to cause an increase of one death amongst those members of the community who have reached thirty years. Then, rising by a definite rule, there have died sixty-four, in proportion to that one, of those who have reached eighty-four years. This is sound calculation, and it leads to reflection. It leads one to ask, What, if the law be so definite, are curative and preventive medicine doing meanwhile, that they shall not disturb it? I fear that they hardly produce perturbations, and I do not see why they should; because, as

the truth opens itself to the mind, the tremendous external change in the forces of the universe that leads to the result, is not to be grappled with nor interfered with by any specific method of human invention. The cause is too general, too overwhelming, too grasping. It is like the lightning stroke in its distance from our command; but it is widely spread, not pointed and concentrate; prolonged, not instantaneous; and, by virtue of these properties, is so much the more subtle and devastating.

At first it seems easy to explain the reason why a sudden fall in temperature should lead to an increase in the number of deaths, and it is to be admitted that, to a certain extent, the reason is clear.

ANIMAL POWER AT DIFFERENT PERIODS OF LIFE.

Without entering on the question whether heat is the animating principle of all living organisms, we may accept that in the evolution of heat in the body we have a measurement of the capacity of the body to sustain motion, which is only another phrase for expressing the resistance of the body to death. For example, if we assume that a healthy man of thirty respires sufficient air per day to produce as much heat as would raise fifty pounds of water at 32° Fahr. to 212° Fahr.; and if we assume that a man of sixty in the same temperature is only able to respire so much air as shall cause him to evolve as much heat as would raise forty pounds of water from 32° to 212°, we see a general reason why the older man should feel an effect from a sudden change in the temperature of the air which the younger would not feel; and if we assume, further, that a man of eighty could in the same time produce as much heat as would raise only twenty pounds of water from

32° to 212°, we see a good reason why the oldest should suffer more from a decrease of external temperature than the other two. It is necessary, however, to know more than this general statement of an approximate fact: we ought to understand the method by which the reduction of temperature influences, and the details of the physiological process connected with the phenomena. When a human body is living after the age when the period of its growth is completed and before the period of its decay has commenced, it produces, when it is quite healthy, by its own chemical processes, so much heat or force as shall enable it, within given bounds, (1) to move its own machinery; (2) to call forth, at will, a limited measure of extra force which has been lying latent in its organism; and, (3) to supply a fluctuating loss that must be conveyed away by contact with the surrounding air, by the earth, and by other bodies that it may touch, and which are colder than itself. There is thus produced in the body, *applied* force, *reserve* force, and *waste* force, and these distributions of the whole force generated, when correctly applied, maintain the perfect organism in such balance that life is true and steady. So much active force carries with it the power to perform so much labour; so much reserve force carries with it the power to perform a measure of new or extra labour to meet emergencies; so much waste force enables the body to resist the external vicissitudes without trenching on the supply that is always wanted to keep the heart pulsating, the chest breathing, the glands secreting or excreting, the digestive apparatus moving, and the brain thinking or absorbing.

Let us, even in the prime of manhood, disturb the distribution of force ever so little, and straightway our life, which is the resultant of force, is disturbed. If we

use the active force too long, we become exhausted, and call on the reserve; if we continue the process, the result is failure more or less perfect, sleep, and, in the end, the last long sleep. Let us, instead of exhausting the force, cut it off at the sources where it is generated; let us remove the carbon or coal that should go in as fuel-food, and we create prostration, and in continuance a waning animal fire, sleep, and death; or let us, instead of removing or withdrawing the supply of fuel, cut off the supply of air, as by immersion of the body in water, or by making it breathe a vapour that weakens the combination of oxygen with carbon,—such a vapour as chloroform,—and again we produce, at once, prostration, sleep, or death, according to the extent to which we have conducted the process. Lastly, if instead of using up unduly the active and reserve force, or of suppressing the evolution of force by the withdrawal of its sources, we expose the body to such an external temperature that it is robbed of its heat faster than it can generate it; if to supply the waste heat we draw upon the active and reserve forces; we call forth immediately the same condition as would follow extreme over-exertion, or suppression of the development of force; we call forth exhaustion and sleep, and, if we go far enough, death.

We have had in view, in the above description, a man in the prime of life, in the centre of growth, and decay. In regard to the force of animation in him, let us look at him now retrospectively and prospectively. In the past his has been a growing, developing body, and in the course of development he has produced an excess of force commensurate with the demands of his growth; this has enabled him gradually to bear more fatigue and more exposure, without exhaustion and even with ease, until he has reached his maximum. When he has stopped in

development, when he stands on a fair level with the external forces that are opposed to him, then his own force, for a short time balanced, soon stands second in command. He feels cold more tenderly; if his rest be broken, the demand for artificial heat is more urgent; if he lose or miss food, he sinks quickly; and, returning to our facts, as to the influence of the external temperature on mortality, these are the reasons why a fall in the thermometer sweeps away our population according to age so ruthlessly and decisively.

If we analyze the facts further by the side of the diseases which kill the old, we find those diseases to be numerous in name, but all of two types. They are diseases which of themselves tend either to produce undue loss of force, or that tend to prevent the development of force at its origin. Thus affections which are accompanied with exhaustive loss of fluids from the body, such as diabetes, dropsies, and hæmorrhages, are of the first class; affections in which due supply of air to the lungs is prevented are of the second class, especially bronchitis, a disease so commonly assigned as the cause of the deaths amongst the members of the aged and enfeebled population, that succeed immediately on an extreme fall of the thermometer.

FALL OF TEMPERATURE—MODE OF ACTION.

In what has been written above I have stated simply and in open terms the fact that the fall of temperature produces a specified series of results, by reducing the force of the living organism, and disposing it to die. We may from this point investigate, from a physiological point of view, the mode by which the effect is produced in the economy. How does the decline of temperature act? Is the process simple or compound?

EXTRACTION OF HEAT.

The process is compound, and into it there enter three elements. In the first place, the body is robbed rapidly of its waste force, and the reserve and active elements of force are, consequently, called upon to the depression of the organism altogether. This obtains because the medium surrounding the body, the air, unless it be artificially heated, removes from its contact with the body a larger proportion of heat than can be spared; and it might be possible to produce such an influence on the body by sudden extraction of its heat as to destroy it at once by the mere act. If a man could be surrounded with frozen mercury he would die instantaneously, as from shock, by the immediate extraction of his heat. But in ordinary cases, and under ordinary circumstances, the mere rapid extraction of waste heat is not sufficient to account for all the mischief produced by a low temperature; for by artificial warmth and non-conducting garments, we counteract the influence, and that, too, in a manner which proves pretty successful. We may, therefore, leave this element of extraction of heat as a most important, but not as the sole, agent of evil.

SUPPRESSED OXIDATION.

The second element is the effect on the process of oxidation of blood under the influence of cold. We all are aware that if a portion of dead animal or vegetable matter be placed at a low temperature, it keeps for a considerable time; and we have evidence of dead animals which, clothed in thick ribbed ice, have been retained from putrefaction for centuries. Hence we say that cold is an antiseptic as alcohol is, and chloroform, and ammonia, and other similar bodies. Cold

is an antiseptic then, but why? Because it prevents, even in the presence of a ferment, the union of oxygen gas with combustible matter. The molecules of oxygen, in order that they shall combine, and in their combination evolve heat, require to be distributed, and to be distributed by the form of motion known as heat: deprive them of this activity, and they come into communion with themselves, are attracted to each other, and lose to the extent of this attraction their power of combining with the molecules of other bodies for which they have an affinity. In an analogous, but more obvious way, we may see the same effect of motion in the microscopic examination of blood. In the blood, while it is circulating briskly in its vessels, there are distributed through it, without contact with each other, the millions of oxygen carriers called blood corpuscles. In the circulation in the free channels of the body, the arteries and veins, it is motion that keeps these corpuscles apart: we draw a drop of blood and let it come to rest on the microscope glass, and as the motion ceases the separated corpuscles run together, and adhere so firmly that we cannot easily separate them without their disintegration. If we were able to drive them in this state round the body, through the vessels, they would not combine readily with the tissues; they have, in fact, forfeited the condition necessary for such combination. So with the oxygen they carry; when its invisible molecules are deprived of the force called heat, which is motion, they do not readily combine with new matter. But perfect combination of oxygen and carbon in the blood is essential to every act of life. In the constant clash of molecule of oxygen with molecule of carbon in the blood lies the mainspring of all animal motion; the motion of the heart itself is secondary to that. Destroy

that union, however slightly, and the balance is lost, and the animal body is, in a plain word, *ill.*

Cold, or decreased temperature, below a given standard, which for sake of comparison we may take at a mean of 40° Fahr., reduces this combination of oxygen and carbon in blood. In my Lettsomian lectures to the Medical Society of London, delivered in 1860, I entered very fully into this subject, and illustrated points of it largely by experiment. Since then I have done more, and although I have not time here to state the details of these researches, I will epitomize the principal facts. I found then that, by exposing blood in chambers into which air can pass in and out, the blood could be oxidized at temperatures of 70° if the distribution of air and blood were effectually secured, and I also found a proper standard of oxidation from a proper temperature. Afterwards I proceeded to test for combination at lower temperatures, and discovered a gradually decreasing scale until I arrived at 40° Fahr., when efficient combination ceased. Of course, my method was a very crude imitation of nature, but it was sufficient to show this fair and reliable result, that the oxidation of blood decreases as the temperature of the oxygen decreases.

From this point I went to animal life itself. I exposed animals to pure cold oxygen and to cold atmospheric air, and compared the results with other experiments in which animals of similar weight were exposed to warm air and warm oxygen. The facts gleaned were most important, for they proved conclusively that the products of combustion, that is to say, the products resulting from the union of oxygen and carbon, were reduced in proportion as the temperature of the oxygen was reduced. In the course of this inquiry another singular and instructive fact was elicited. It has been long known that at

ordinary temperature, say 60°, pure neutral oxygen does not support animal life so well as oxygen that is diluted with nitrogen. In the nitrogen the molecules of oxygen are more freely distributed under the influence of motion, that is the meaning of the observed fact. What, then, would be the respective influence of low and high temperatures on the respiration of pure oxygen? To settle this question, animals of the same size and weight were placed in equal measures of oxygen gas and common air at a temperature of 20° Fahr., and with the inevitable result that the animal in the pure oxygen ceased to respire one-third sooner than did the animal in common air. Carrying the inquiry further, I found that if the oxygen gas were warmed to 50° Fahr., the respiration was continued six times as long as in the previous experiment, while if the warming were carried to 70° it was sustained twenty-four times as long. I reversed the experiment; I made oxygen with cold produce anæsthetic sleep in a warm-blooded animal.

I need not carry this argument further; it is the easiest of the demonstrative facts of physiological science that reduction of temperature lessens the combining power of oxygen for blood, and therewith causes a reduction of animal force, and a tendency to arrest of that force, which, in the end, means *death*.

MECHANICAL COLD.

The third element in the action of cold is more purely mechanical, and this, though in a sense secondary, is of immense import. When any body, capable of expansion by heat, that is to say, by radiant motion of its own particles, is reduced in temperature, it loses volume, contracts, or shrinks. The animal body is no exception to this rule: a ring that will fit tightly to the warm finger

will fall off the same finger after exposure to cold. The whole of the soft parts shrink, and the vessels contract and empty themselves of their blood. Cold applied to the skin in an extreme degree blanches the skin, and renders it insensible and bloodless, so that if you prick it it does not bleed, neither does it feel. In cases where the body altogether is exposed to extreme cold this shrinking of the external parts is universal: the whole surface becomes pale and insensible; the blood in the small vessels superficially placed is forced inwards upon the heart and vessels of the interior organs; the brain is oppressed with blood; sleep, or coma, as it is technically called, follows, and at last life is suspended.

In exposure to the lowest wave of temperature in this country these extreme effects are not commonly developed; but minor effects are brought out which are most significant. In particular, the effect on the lungs is strongly marked. The capillary vessels of the lungs making up that fine network which plays over the computed six hundred millions of air vesicles, undergo paralysis when the cold air enters, and in proportion as such obstruction from this cause is decisive, the blood that should be brought to the air vesicles is impeded, and the process of oxidation is mechanically as well as chemically suppressed. The same contraction is also exerted on the vessels of the skin, driving the blood into the interior and better protected organs. Hence the reason why on leaving a warm room to enter a cold frosty air there is an immediate action of the visceral organs from pressure of blood on them, and not unfrequently a tendency to diarrhœa from temporary congestion of the digestive tract. Three factors are at work, in fact, whenever the low wave of temperature affects the animal body: abstraction of heat from the body, beyond what is natural; arrest of chemical action

and of combustion; paralysis of the minute vessels exposed to the cold.

COMBINED EFFECTS.

We cannot view the extent of change in the organic life induced by the low wave of heat without seeing at once the sweep of mischief which exposure to the wave may effect. It exerts an influence on healthy life in the middle-aged man, and I know of no disease which it does not influence disastrously. Is the healthy man exhausted, it favours internal congestion; has he a weak point in the vascular system of his brain, it renders that point liable to pressure and rupture, with apoplexy as the sequence; is he suffering from bronchial disease, and obstruction, already, in his air passages, here is a means by which the evils are doubled; has he a feeble, worn-out heart, it is unable to bear the pressure that is put upon it; has he partial obstruction of the kidney circulation, he is threatened with complete obstruction; is he indifferently fed, he is weakened generally. It is from this extent of action that the mortality of all diseases runs up so fast, when the low wave of heat rolls over the population, affecting, as we have seen, the feeblest first.

Another danger sometimes follows which is remote, but may be fatal, even to persons who are in health. It is one of the best known facts in science that when a part of the surface of the body has been exposed long to cold, the greatest risk is run in trying suddenly to warm it. The vessels become rapidly dilated, their coats relax, and extreme congestion follows. But what is true of the skin is true equally, and with more practical force, of the lungs. A man, a little below par, goes out when the wave of temperature is low, and feels oppressed, cold, weak, and miserable; the circulation through his lungs has

been suppressed, and he is not duly oxidizing: he returns to a warm place, he rushes to the fire, breathes eagerly and long the heated air, and adds to the warmth by taking perchance a cup of stimulant; then he goes to bed and wakes in a few hours with what is called pneumonia, or with bronchitis, or with both diseases. What has happened? The simple physical fact of reaction under too sudden an exposure to heat after exposure to cold. The capillaries of the lungs have become engorged, and the circulation static, so that there must be reaction of heat, inflammation, before recovery can occur. Nearly all bronchial affections are induced in this manner, not always nor necessarily in the acute form, but more frequently by slow degrees, by repetition and repetition of the evil. Colds are often taken in this same way, from the exposed mucous surfaces of the nose and throat being subjected first to a chill, then to heat.

The wave of low temperature affecting a mixed population finds inevitably a certain number of persons of all ages and conditions on whom to exert its power. It catches them too often when they least expect it. An aged man, with sluggish heart, goes to bed and reclines to sleep in a temperature, say, of 50° or 55°. In his sleep, were it quite uninfluenced from without, his heart and his breathing would naturally decline. Gradually, as the night advances, the low wave of heat steals over the sleeper, and the air he was breathing at 55° falls and falls to 40°, or it may be to 35° or 30°. What may naturally follow less than a deeper sleep? Is it not natural that the sleep so profound shall stop the labouring heart? Certainly. The great narcotic never travels without fastening on some victims in this wise, removing them, imperceptibly to themselves, into sleep ending in absolute death.

SOME SIMPLE RULES.

The study of the physiological influence of the wave of low temperature, and of its relation to the wave of mortality, suggests a few rules, simple, and easily remembered.

1. Clothing is the first thing to attend to. To have the body, during variable weather, such as now obtains, well enveloped from head to foot in non-conducting substance is essential: who neglects this precaution is guilty of a grievous error, and who helps the poor to clothe effectively does more for them than can readily be conceived without careful attention to the subject we have discussed.

2. In sitting-rooms and in bedrooms it is equally essential to maintain an equable temperature; a fire in a bedroom is of first value at this season. The fire sustains the external warmth, encourages ventilation, and gives health not less than comfort.

3. In going from a warm into a cold atmosphere, in breasting the wave of low temperature, no one can harm by starting forth thoroughly warm. But in returning from the cold into the warm the act should always be accomplished gradually. This important rule may readily be carried in mind by connecting it with the fact that the only safe mode of curing a frozen part is to rub it with ice, so as to restore the temperature slowly.

4. The wave of low temperature requires to be met by good, nutritious, warm food. Heat-forming foods, such as bread, sugar, butter, oatmeal porridge, and potatoes, are of special use now. It would be against science and instinct alike to omit such foods when the body requires heat.

5. It is an entire mistake to suppose that the wave of cold is neutralized in any sense by the use of alcoholics. When a glass of hot brandy-and-water warms the cold

man, the credit belongs to the hot water, and any discredit that may follow to the brandy. So far from alcohol checking the cold in action, it goes with it, and therewith aids in arresting the motion of the heart in the living animal, because it reduces oxidation.

6. Excessive exercise of the body, and overwork either of body or of mind, should be avoided, especially during those seasons when a sudden fall of temperature is of frequent occurrence. For exhaustion, whether physical or mental, means loss of motion in the organism; and loss of motion is the same as loss of heat.

One further consideration, suggested by the subject of this paper, has reference to the bearing of the public towards the labours of the medical man in meeting the effects of the low wave of heat. The public, looking on the doctor as a sort of mystical high priest who ought to save, may often be dissatisfied with his work. Let the dissatisfied think of what is meant by saving when there is a sudden fall in the thermometer. Let them recall that it is not bronchitis as a cause of death, nor apoplexy, nor heart disease, as such, that the doctor is called on to meet; but an all-pervading influence which overwhelms like the sea, and against which, in the mass, individual effort stands paralysed and helpless. When the doctor is summoned the mischief has at least commenced, and, it may be, is so far over that treatment by mere medicines sinks into secondary significance. Then he, true minister of health, candid enough to bow humbly before the great and inevitable truth, and professing no specific cure by nostrum or symbol, can only try to avert further danger by teaching elementary principles, and by making the unlearned the participators in his own learning.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

A NEW MODE OF ARTIFICIAL RESPIRATION FOR ANÆSTHESIA.

IN working with the feather valve inhaler lately brought out by Messrs. Krohne & Sesemann, a modification of it occurred to me, which will, I think, render it the most perfect inhaler that has yet been constructed, since it enables the administrator of a narcotic vapour,—of methylene, chloroform, or ether,—to stop the supply of the narcotic by simply turning a tap, and still using the bellows, which he holds in his right hand, to set up, without a moment's loss of time, artificial respiration.

It will be remembered by those who are familiar with the inhaler first devised by Dr. Junker, that the narcotic vapour is delivered into the mouthpiece by air driven through narcotic liquid contained in a small Wolff's bottle, suspended to the lappet of the coat of the administrator; and that each stroke of a pair of little hand bellows, connected with the descending tube of the bottle, sends a small percentage of the vapour into the mouthpiece for the patient to inhale. The apparatus, therefore, now consists of a pair of hand bellows, connected with the entrance tube of a Wolff's bottle,

an exit tube from the bottle to a mouthpiece, and a mouthpiece covering both the mouth and nostrils, armed with a feather exit valve, which by its vibration gives constant evidence of the character of the respiration.

My modification and addition to the apparatus consists in putting a shunt tube to carry the air from the bellows to the mouthpiece directly when necessary. A stopcock at the point of juncture of the two tubes from the bellows determines, according to the manner in which it is set, whether the air driven along by the bellows shall make its way through the narcotic fluid to the mouthpiece, or whether it shall go directly to the mouthpiece as pure air. This is one modification; a second is, that over the expiratory valve there is a movable cover or lid, which can be closed down in an instant at pleasure. Lastly, the mouthpiece is well padded, in order to make it fit closely to the face, should occasion require.

The application of this inhaler is as follows. For narcotizing, the tap is turned so as to admit the air injected by the bellows through the narcotic only into the mouthpiece, the cover over the expiratory valve being left open. The vapour is, in this manner, administered until anæsthesia is induced, and, if all goes well, nothing more is demanded. If, however, the feather valve shows respiratory failure the stopcock is turned, so as to shut off the narcotic vapour and allow the bellows to throw pure air into the mouthpiece. Should the respiration stop, the administrator has only to shut down the cover or lid over the expiratory valve, to hold the mouthpiece firmly down to the face, and to continue the action of the bellows. By this method he can inflate the chest of the patient with pure air, and by removing the mouthpiece for one or two seconds can permit the lungs to empty themselves by their

natural contraction. Each stroke of the bellows supplies five cubic inches of air; and, for an adult, six such strokes are sufficient to fill the chest with as much air as is required for one inspiratory act. So, the process is, to pump six strokes with the bellows, the mouthpiece being held firmly to the face with the left hand; to lift up the mouthpiece whilst *six* is deliberately counted; to replace the mouthpiece, refill the chest with six charges of air from the bellows, and so on in steady repetition.

The advantages of this method are: (*a*) its simplicity; (*b*) its readiness, no new apparatus being required; (*c*) the instantaneous ease with which it can be applied on the least sign of danger. In a word, artificial respiration is a continuation of what is being done, and is as facile to the administrator as the working of the lever is to the engineer. Mr. Krohne has very faithfully carried out my instructions in all the details supplied above, and I have no doubt as to a good result from the application of the method in anæsthetic practice.

HÆMORRHAGE AFTER TOOTH EXTRACTION, AND IMPROVED NATIONAL BLOOD.

THE accident of hæmorrhage after tooth extraction is much less frequent than it was a quarter of a century ago. At that time it was, comparatively, a common accident; at the present time it is a comparatively rare accident. For my own part I have never had a case which ended fatally, although two or three have approached very near to a fatal termination; but I knew, before the period I have named, of more than one death from the hæmorrhage.

For years past I have not known of one fatality, and the occurrence of bad cases is much limited. There is not the least reason for surprise at this fact, for many causes are at work leading to it. In the first place, the progress of dental surgery is greatly advanced, and the rude method of operating with the key or with coarsely constructed forceps has ceased. Secondly, the knowledge that different teeth call for different manipulation or different instruments, knowledge once altogether confined to a limited few, is now general knowledge and practice. Thirdly, the means to be adopted to stop hæmorrhage, if it be developed, is much more accurate than it used to be. The operator has gained confidence, and, with that, coolness and patience in endeavouring to arrest the flow of blood—elements of treatment of the supremest importance. Beyond these advancements there is yet another, which probably ought to stand first, namely, that what we physicians call the hæmorrhagic diathesis is greatly reduced amongst the members of the community. This is one of the most striking facts of our civilization—so striking I am rather surprised it should have been left to me to call attention to it. In plain words, the blood of the nation has improved in quality. What was termed scurvy was not long ago a prevailing condition. There was land scurvy and there was sea scurvy. Now both are much less common. But the scorbutic state, induced in all cases by an improper diet, or descending by heredity from parents who had been badly fed and badly nourished, meant above all things else poor blood, blood that would not coagulate or seal up the broken vessel. Such blood was made by bad food; it was also made by bad drink, for it was the blood also of an intemperate community; and, bad food and bad drink having to some extent been dismissed and

replaced by wholesomer substances, the blood is of better substance, is a sealing blood, and ceases to flow out of minute vessels less seriously, because it makes for itself the best of all plugs in its semi-solid or coagulated form.

TREATMENT OF HÆMORRHAGE AFTER TOOTH EXTRACTION.

IN my early practice, my instant attempt in cases of hæmorrhage after tooth extraction was to stem the flow of blood, without enquiring too minutely as to cause. By-and-bye I found that there was rarely any great reason for hurry; that it would take a long time for a person to die from the loss of blood; and, that faintness was not so serious as some thought it, but rather assisted the action of remedies, because it relieved the pressure of the circulation, and made it easier to manipulate on the point from which the hæmorrhage proceeded. Gradually I found certain reliable steps, which I now follow, and which I think should always be followed.

In the first place, it is essential to ascertain if the blood that is being lost is or is not coagulable. If the blood is coagulable, if a little of it caught in a cup or glass goes into a firm clot in the course of two or three minutes, much cause for alarm is removed, and the line of treatment is sufficiently clear. We may be sure in such a case that the cause is either vascular or mechanical, that there is either failure in the contraction of the bleeding vessels, or adhesion of the bleeding vessel to the surrounding bone tissue, or rupture of an abnormally large vessel. Whichever it may be, there is the assurance at hand that the blood is in a favourable condition for forming a plug, and that if it can be retained in the cavity a sufficient

time to enable it to form a firm clot it will produce a natural arrest.

This treatment includes one of three methods. (1) A styptic. (2) A plug with pressure, with or without styptic. (3) The cautery. In the beginning of my practice I trusted mainly to styptics in this class of case, and after many trials I came to the conclusion that nitric acid was the best. I believe still, that as a styptic it is one of the best, but it has the disadvantage of being very difficult to manipulate, owing to its extreme caustic and destructive properties. I afterwards invented styptic colloid, which is composed of tannin in collodion, with addition of tincture of benzoin; and, for ordinary cases, I do not know of a better remedy. The colloid could be applied on cotton wool, and a firm plug of it could be inserted with much facility. When this did not seem sufficient I resorted to perchloride of iron, applied in solution with cotton wool. It has often happened that these simple methods have proved effective; when they have not I have proceeded to the second plan, that of the plug with pressure.

Three kinds of plugs have been used. One is made of soft, well-teased cotton wool saturated with styptic colloid. The mass is not put in in too moist a form, but, mixed in a cup, is allowed slightly to dry by the evaporation of the ether. Then, after well drying the bleeding cavity with a pledget of lint, the colloid cotton is inserted with forceps, the points of which have been dipped in olive oil, layer by layer, as in a process of roughly stopping a tooth, until the cavity is completely filled. A little bit of dry wool is next made to cap the whole, and pressure is applied with the finger or with Harding's compress until the plug is solid, which, owing to the escape of ether, is rather a quick process.

A second plan is to use a perchloride of iron plug, made by saturating cotton wool with solution of perchloride of iron, and then drying the cotton down. Cotton so prepared can be kept in bottles ready for use, and can be applied as a plug in a similar manner as the colloid. I have also constructed what may be called the *wick styptic*, and this is very useful in minor cases. It consists of a glass tube having a narrow point, through which is drawn a piece of cotton wick saturated with perchloride. A portion of the wick projecting from the narrow end of the tube can easily be inserted and firmly held in the cavity, and if it becomes saturated with blood it can be cut off, a new bit pulled out, and a fresh application made.

But the best plug of all which I have been able to construct is a gutta-percha styptic plug. It is very convenient, and is easily kept at hand for an emergency. The plug is made from a gutta-percha disc saturated with styptic, tannin or perchloride of iron. These discs, picked up by the forceps and held for a short time in hot water, soften and become so malleable that they can be inserted into any cavity. The bleeding tooth cavity is quickly dried with absorbent cotton wool, and is then filled with the plastic styptic, pressed firmly down to the bottom, and held there until it becomes firm. There is no fear of inserting the plastic styptic too hot, since heat favours coagulation.

Presuming that the blood flowing from the injured part shows no tendency to clot, and is discharged in a steady stream, which is not arrested by pressure or the ordinary application of a styptic, it is necessary at once to produce secondary or albuminoid coagulation. It must be remembered that no blood is so constituted but that it can be made to coagulate. In the most fluid blood

there is albumen, which will not solidify spontaneously, but which can be solidified by various means artificially. Three of these means we have at hand, namely, tannin, perchloride of iron, and the subdued actual cautery. It may be well always to try the two first-named styptics, and in the perchloride percha styptic and in the percha tannin styptic we have to some extent substances which will coagulate albumen, even when they are not heated, and will instantly cause coagulation if heated to 150° Fahr. They, therefore, are very ready in emergency. If they fail, no time should be lost in resorting at once to the cautery. When the electric cautery is at hand it is the most convenient; it does not alarm the patient, and the heat of the terminal probe is sustained in the cavity; but if the electric cautery be not at hand, the next best is the small iron cautery, an iron bulb terminating in a rounded point, and fixed in a strong handle at an oblique angle. This bulb and point can be made red-hot in the flame of a spirit lamp or of a gas jet, but it is not necessary to alarm the patient by using it while it is in a state of glow. It may be allowed to cool down until all redness has disappeared, and can then be inserted deep down into the bleeding cavity. Skilfully used it gives no pain of moment, and its effect is, as a rule, immediate in the arrest of blood. Should it fail, there is no other direct local measure that can be depended upon. Even ligature of the carotid, if the case should come into the hands of the operating surgeon, would be a doubtful measure, since hæmorrhage from the surgical wound might be uncontrollable, and simply add to the danger. Happily, for me, the danger has only once pressed to the extent of suggesting so extreme a measure, and then the hæmorrhage was, at last, stopped by the cautery, and did not return. In other cases, four in number, the

cautery has acted at once. It is a remedy applied with unusual readiness if the idea and fear of it be not exaggerated. It should never be used at such a heat as to destroy structure. The coagulating power of heat over albumen, viz., 140° F., is all that is required strictly; but this may be safely exceeded to 50 or 60 degrees.

AVOIDANCE OF STIMULANTS DURING HÆMORRHAGE.

IT is customary when the accident of hæmorrhage occurs for the operator, or some bystander, to administer wine, brandy, or some other alcoholic stimulant to the patient, under the false idea of sustaining the vital power. It is my solemn duty to protest against this practice on the strictest and purest scientific grounds. The action of alcohol, under such circumstances, is injurious all round. It excites the patient, and renders him or her nervous and restless. It relaxes the arteries, and favours the escape of blood through the divided structures. Entering the circulation in a diluted state, it acts after the manner of a salt in destroying the coagulating quality of the blood; and above all other mischiefs, it increases the action of the heart, stimulating it to throw out more blood through the divided vessels. These are all serious mischiefs, but the last named is the worst. In hæmorrhage the very keystone of success lies so much in quietness of the circulation that actual failure of the heart, up to faintness, is an advantage, for it brings the blood at the bleeding point to a standstill, enables it to clot firmly, when it has that tendency, and forms the most effective possible check upon the flow from the vessels. In a case where I had to remove a hard palate, that had become carious,

in order to reach and twist a bleeding vessel, although the quantity of blood lost during the hæmorrhage amounted to over three pounds in weight, and the syncope was so extreme the patient did not know anything had been done to him after he returned to consciousness, not a drop of stimulant was administered at any stage, and the care to avoid such administration was carefully sustained after recovery, in order that the rapid action of the heart might not overcome the resistance of the tenderly sealed up vessel. I am certain that this rule of avoiding stimulation was far more effective in saving my patient's life than any surgical skill of mine, for the vessel I twisted might have become plugged up, naturally, during the syncope, but nothing would have prevented the bleeding from breaking out afresh if the heart, instead of being allowed to swing round of itself, slowly and safely, had been whipped, for a brief period, into violent action. I refer to this case as typical, because if a stimulant were not wanted in it, a stimulant cannot be called for in examples less severe. The course followed was simply to lay the patient quite recumbent when signs of faintness supervened, and, so long as he could swallow, to feed him with warm milk and water freely. Such, in my opinion, is the proper treatment to be employed in every instance of syncope from loss of blood.

EFFECT OF SWALLOWING BLOOD DURING HÆMORRHAGE.

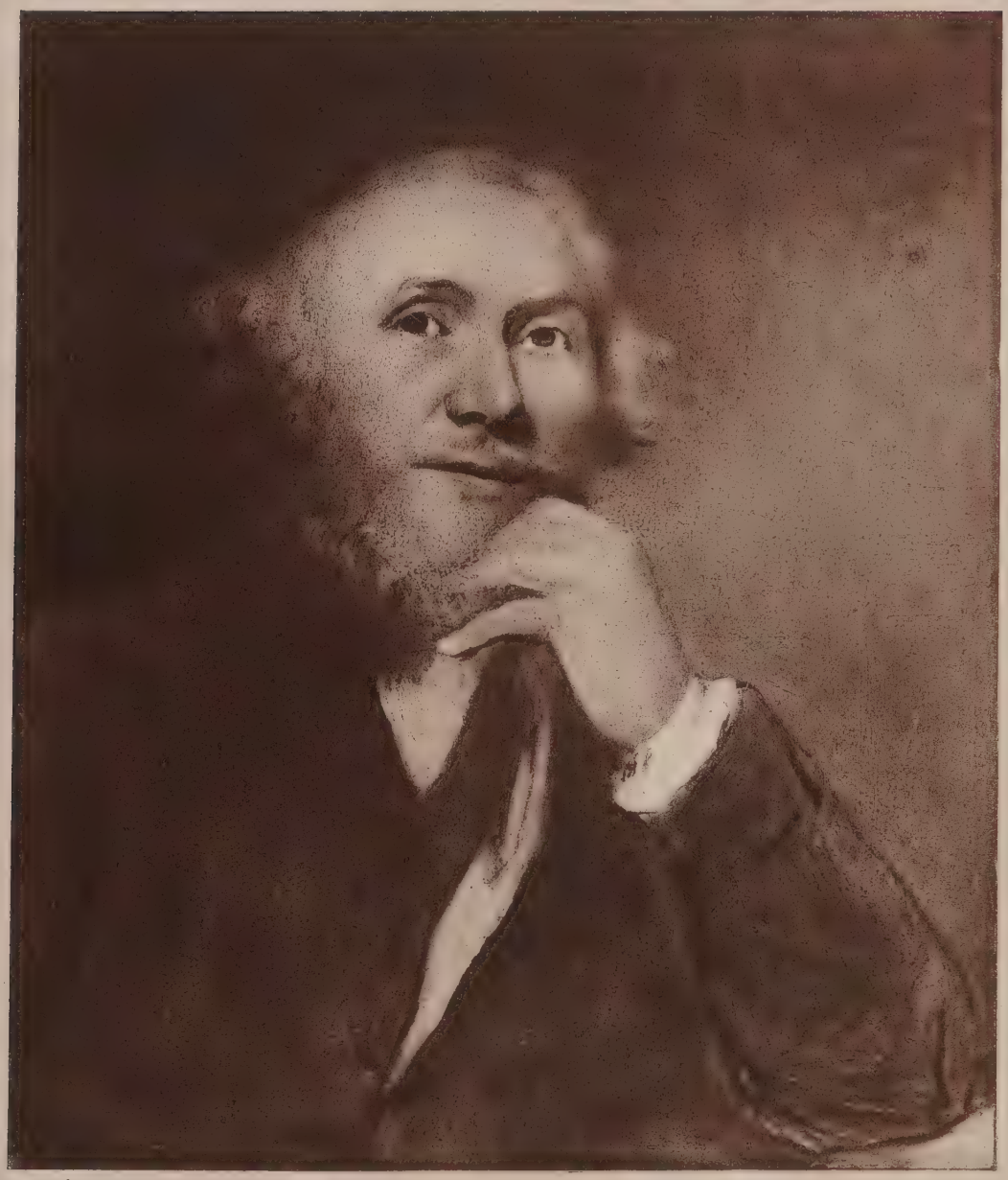
IN all bad cases of hæmorrhage from the mouth some blood is, almost of necessity, swallowed. The act sometimes gives cause for undue alarm, and it invariably alarms the patient. At first I was a prey to some alarm on that score, but experience has shown that the anxiety is groundless. The

fact of a person swallowing a good many ounces of blood is not necessarily injurious. The blood is digested, or if in too large a quantity, is vomited, with return of faintness, from which, however, I have never observed serious danger or recurrence of hæmorrhage.

PERCHA DRESSINGS, STYPTIC AND ANTISEPTIC. A NEW USE FOR GUTTA-PERCHA IN PRACTICE.

THE notice given above of a gutta-percha styptic plug deserves a little further elucidation. I have found that gutta-percha, by being softened almost to fluidity in hot water, to which a little glycerine has been added, can be made to take up not only tannin and perchloride of iron, but benzoin, carbolic acid, mercurial salts, and many other styptic and antiseptic substances. After it is saturated with the substance added to it, it is allowed to cool until it becomes simply a soft structure, that can be moulded into plates, discs, or pellets, which soon become hard and remain ready for use. These keep for any length of time. When required they are softened by immersion in hot water, and can then be moulded, according to the case, into plugs, splints, or coverings of dressings. The percha can also be drawn out, under warmth, into a fine tissue, and used like adhesive plaster, as round a cut finger; where, left to harden, it becomes both plaster and splint.

I have tried to saturate the gutta-percha with styptics and antiseptics, by dissolving it first in volatile chemical solutions, and then admixing and evaporizing; but I prefer heating it simply in water, or in water with glycerine.



Asclepiad No. 29. Vol. VIII. London, February 1891.

John Hunter

Autotype from a portrait by
Sir Joshua Reynolds, in the possession of
the Society of Apothecaries London.
B.W.R.

JOHN HUNTER AND THE SCHOOL OF HUNTERIAN MEDICINE.

IN British surgery the name of John Hunter eclipses every other individual name, and to a certain extent all names put together. It is a name that stands out alone, and that actually bears idolatry without being injured. Year by year, on Hunter's natal day, some surgical voice shakes the air in his praise, now in strains of worship that are intelligible; then unintelligible, in so far as the man is concerned, as if he were a god shrouded in mystery, and had only to be reverentially mentioned, or used as a peg for the orator to hang his own thoughts on—for richer, for poorer—in relation to some current professional topic, commonplace or otherwise, which may be standing out as the talk of the passing day. How his perturbed spirit, if it should linger still about the Museum he founded, takes to these annual forms of idolization, who shall tell? In his wild-oats days—for under the title of "Jack" Hunter he was, for a few years, a great sower of that particular grain—he swore roundly, and indeed never got out of the habit to the last of his days. What, now, if his perturbed spirit, under sore temptation, should break forth during an oration and express its mind! One would like to see the consternation, with the idol at large and giving tongue.

AT HOME IN LONDON.

To get at the man and not the idol let us go back in history one hundred years, say to New Year's Day, 1791. Let us locate ourselves in London, and proceed to Leicester Fields,—now known as Leicester Square.

28, LEICESTER SQUARE, 1891.

On the east side of the square, at No. 28, we shall reach the house of Hunter; a house, we shall find, measuring about twenty-eight feet of frontage, with premises running back to Castle Street; a house of four stories, with four windows on each floor, and a side door on the left hand; a good roomy house, in a fine central position, and

behind it the famous Museum, not very long removed from Jermyn Street, and already the grand natural history collection of the world. As the time of our supposed visit is in midwinter, the chances are that all the Hunter family will be in residence in this house. There will be Anne Hunter, the beautiful wife, sister of Everard Home, and daughter of Surgeon Home, formerly of Burgoyne's regiment of light horse; John Hunter junior —"little John," and Agnes his sister; two children out of four, two others having died during childhood. Mrs. Hunter has been married to the great man, her husband, nearly nineteen years, but retains her beauty, for which she has long been remarkable, and which, doubtless, won the heart of her spouse, since silver and gold she had none; a fact which prudent brother William Hunter did not particularly relish. To her beauty Mrs. Hunter adds industry; she is a queen of industry. To industry she adds genius, genius for poetry of no mean order; she writes words for the canzonets of Haydn, and some songs, one of which, beginning—

"My mother bids me bind my hair,"

takes its place among the classics, and promises to continue to hold its place, perhaps as long as the writings of the master of the Museum themselves. Lastly, Mrs. Hunter is a woman of fashion, a leader of society, who has what John, her husband, calls "kick-ups," sometimes even on Sunday night. John himself views these kick-ups with pleasure or displeasure, according to his humour. Sometimes his house apprentice, William Clift, will have to stay to witness, with sleep-laden eyes, the hour when the master, no longer able to dictate, will issue from his study on the ground floor, during one of Mrs. Hunter's reception nights, to seek his much-needed repose, and, with

difficulty meeting the social stream on the staircase, will stop to extend a kindly greeting to the beauty of the year, give a smart reply to the passing joke of the man of fashion, or a more serious response to the question of an administrator. At other times he will not be so complaisant. One night, coming home fagged and weary, to discover his drawing-room filled with "musical professors, connoisseurs, and other idlers," whom Mrs. Hunter has assembled, he will go into one of his passions, and address the astonished guests with a bit of his mind. "I knew nothing of this kick-up, and I ought to have been informed of it beforehand; but as I have now returned home to study I hope the present company will retire." Whereupon, as Drewry Ottley says, "Exeunt Omnes."

We may form some idea of the man himself from these particulars. An abrupt man, in manner sharp and impetuous; a man who stands no nonsense, and who makes all about him subservient to his work. Wife, friends, pupils, servants, even to the coachman, must, *nolens volens*, help him in the scientific work of his life; and, though it may shorten his life, he will have his way. As to the man corporeally, he is of middle height, of strong frame, but with high shoulders; his countenance firm, and yet not forbidding; his forehead well formed, his eyes small, and of light blue tint, with eyebrows large; his cheekbones high, his lips strong; his moustache and beard rugged and scanty; his locks, of reddish tint once, now grey, thin, and carelessly trimmed; his dress good but careless; in face, figure, and style, forsooth, a workman throughout; which word "workman" explains the man better than any other, for a workman he is, if ever there were one in all time. By work he has won everything he has and possesses. He has gathered round him a few friends who are devoted to him, but with his profession

at large he is not what is called popular. The masses of the profession do not understand him; never dream of him as a man who may one day find entrance into Westminster Abbey and have an oration spoken in his honour regularly each year. The courtly people, moreover, seem to have him little in view, and without any doubt think far more of the beautiful and accomplished Anne, with her kick-ups, than of John with his wet anatomical preparations, his dry bones, his books, and his bad tempers. Yet, in spite of every obstacle, he has won his way by work, earns a fair professional income, and, not caring for such an event, but holding that "no man ever was a great man who wanted to be one," has done enough to make his name immortal. In a word, he is irrepressible, and thinks only of work. Now and then he may go to his club for an evening, or to the theatre to see Mrs. Siddons; nay, he may take part in a dance, or entertain a party of congenial friends, merrily, at the dinner-table; but these escapades mark an occasion not often to be repeated, and a wonder when they are known to his house apprentices and the other members of his circle, all of whom have to go as fast as a vehement man who hates butter-fingered people, reckons up time moment by moment, has no mercy for loiterers, expects to be obeyed the instant he has given his commands, and breaks out occasionally into such storms of passion that he feels they may bring his life to an instant termination,—as fast as he can drive them.

At this date the life of John Hunter, though it is merely a life of sixty-two years, hangs on a thread, and he knows the fact right well. His heart is all wrong. He has seizures of what the learned are beginning to call angina pectoris. In the seizures he feels as if his body were reduced in size, and he suffers acutely. In spite of

this he holds on at his labours, multifarious as they are, without hesitation, pause, or admission of fatigue. He has tried repose for short periods, and found it irksome. He has tried change of air for some weeks at Bath, and that, too, is irksome. So he keeps at his work, and never admits he is weary of it. He begins his work early. "Come to me as early as you can," he says to a visitor who desires to see the Museum. The visitor, taking him at his word, goes at five in the morning, and there meets the master ready to receive him. After two or three hours' work in the early morning, breakfast is quickly taken at nine o'clock, and the professional labours commence. Long morning consultation practice at home is not the fashion at this period. The carriage comes to the door at eleven, and, with a copy of his engagements in hand, off he starts, leaving the original list of engagements behind him for reference, if he should, in the meantime, be wanted. His engagements will include private attendances, consultations, hospital practice at St. George's, lectures, and other duties, all carried out with such order and regularity that let him who dare interrupt them, even for a moment. Poor Surgeon Cline, a warm Hunterian, knows this fact to his cost. Cline has arranged for Mr. Hunter to see a patient with him after midday, but during his rounds in the morning makes, thoughtlessly, another appointment for him with another patient later on. When, therefore, the first consultation is over, Cline mentions the second, to bring down on his unfortunate head a storm of rage and fury which astounds him. Of course he has his excuses and his apologies, and at last succeeds in bringing the infuriated man of order to say, "Come along, then, let us go and see our patient."

At four in the afternoon Mr. Hunter is back to dinner; and let there be no mistake about it, dinner must be ready

and on the table at that hour. Whether he be at home or not, dinner must be on the table at four. After dinner he has a sleep, which is to him a wonderful restorative; and the sleep over, the delightful hours come when he holds his communion with nature, in the face of her works collected from all parts of the world by every hand that can be pressed into the service, and with printer and printing press at hand, on the premises, ready to fix every good observation. Interruption in these hours is heart-breaking. "Well, Lynn" (to an assistant), "I must go and earn this damned guinea, or I shall be sure to want it to-morrow."

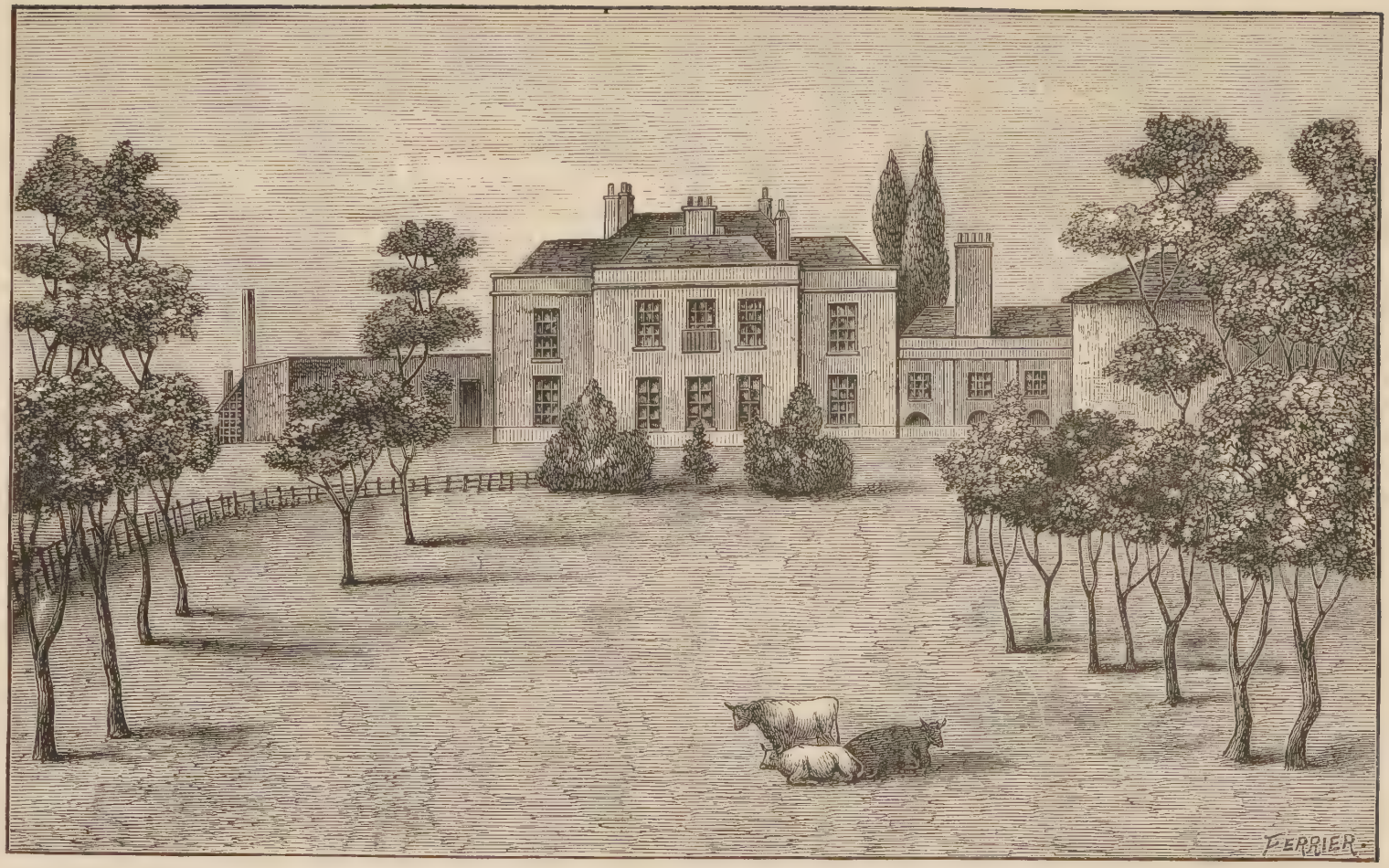
The incident throws another light on the character of Hunter in regard to money and his own pecuniary resources. He has no care for money whatever, except in so far as it helps him to pursue his scientific work, and to add to the science of the world. Born poor, and earning everything for himself, he has lived, so to speak, and not to speak disrespectfully, in a luxurious way, from hand to mouth; for although his income is large, and he is a good pay-master, the cost of the Museum keeps him always close run, and so poor that if he should die Mrs. Hunter would be, to speak the plain truth, in needy circumstances. Nor is this wonderful, considering that the Museum from first to last has cost its founder no less a sum than seventy thousand pounds.

The Museum at the back of the house in Leicester Fields is the best-beloved home of John Hunter. Here, with his artist-assistant Bell, whose pen and pencil are alike facile, with Home, Lynn, Clift, and his few other favourite pupils, he falls into those abstracted moods when all his parts—body, mind, and spirit—are, with one intent, employed in unravelling the deep and subtle and yet doubtless simple problems which lie before him. Beddoes,

usually just, is too hard in judgment when he says "John Hunter fancies that what he cannot find words for is very profound. He is mistaken; whenever he finds himself at a loss for words he is labouring with the delivery of nothing." Beddoes errs on this point. John Hunter, discoursing to his students in class in his Museum, is no orator, is little gifted with power of exposition, is sometimes coarse in his language, is apt to show that he is poorly read, and now and again is led to speak in self-originality which to him is truly original, but which has been said before; all passing faults, some of those spots on the sun which Mrs. Hunter recognized and admitted. But he never "delivers himself of nothing." With all his faults, whenever he is at work in his Museum he is teasing out nature, and, with hands as skilful as laborious, is fixing her designs, for the benefit of future generations.

AT HOME IN HIS COUNTRY BOX.

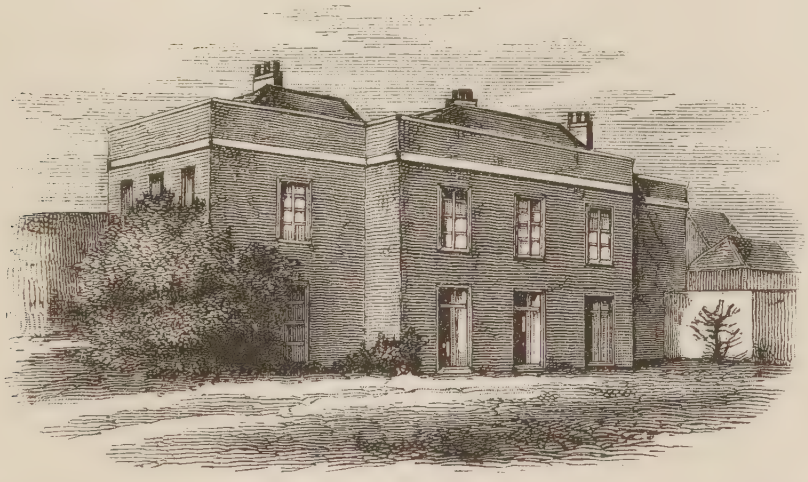
We are now a little acquainted with John Hunter at home in Leicester Fields, but to know more of him we must follow him out of London into what he calls his "country box," situated at Earl's Court, Kensington. It is a pleasant hour's drive across country from Leicester Fields to this quiet and secluded suburb. The road will skirt Hyde Park, will run through Knightsbridge, and along a good path to the Court. "The Box" stands in its own grounds, a little garden in front of it, a large meadow behind, as shown in the drawing opposite. We enter by the meadow through two large gates swinging from sturdy pillars of brick in the Queen Anne style, and crossed by an arch, also of brick. The meadow is quite an expanse, with rows of trees on each side, and with the house not precisely in the centre, but lying



HUNTER'S HOUSE AT EARL'S COURT, KENSINGTON, FROM THE MEADOW, 1764-93.

(*After a sketch of the period.*)

rather to the left hand. A broad drive leads round by the right hand to the front door of the house; a pretty box in every sense of the word. Suppose it be now midsummer instead of midwinter, the Hunter family will have migrated to this nook out of town. Little John and Agnes will have their playgrounds here, and Anne, their mother, will have her days to herself, while her imperious lord and master is doing his London work, going out to it early and coming home early, again to work; for be assured Earl's Court House is no bower of



HUNTER'S HOUSE AS STANDING IN 1886. FROM THE MEADOW.

idleness, or anything of that sort. Mrs. Hunter has made the house beautiful. She has decorated it partly in the Egyptian style, then a prevailing fashion or taste, which the visitor cannot fail to admire. Her own boudoir is a perfect picture, the panel of every door a little gem, a coloured sketch on a white enamelled ground. The ceiling of the drawing-room is richly ornamented with strange figures of strange birds, which husband John must have laughed at, but never dared to classify; while on movable panels on the walls are exquisite water-coloured sketches of the story of Cupid and Psyche. The gardens outside are rich also in colours of birds

and fruits and flowers. All this and much more is according to the fine taste of the poetess wife, the Eve of the garden in the artistic sense; but her Adam is more than half owner, and for his purposes the place has anything but a paradisiacal appearance, unless we accept that all the creatures there are tame, and that the leopards and the lambs lie down together; a supposition not sustained by the facts presented to the world at Earl's Court; for Hunter's gardens here are zoological gardens,—gardens in which he rambles at night studying

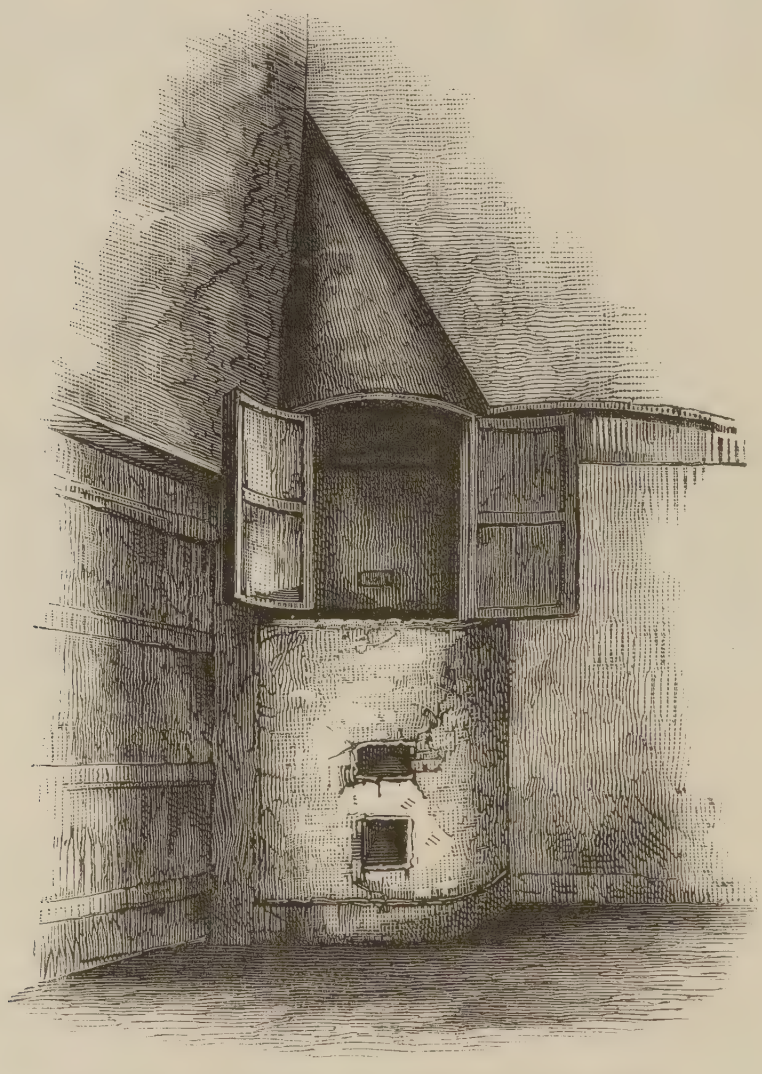
The Lions' House as standing in 1886.

the ways and manners of the varied forms of life in every possible modification of such life, from the smallest to the grandest types of it. At the top of the meadow is the lions'-den, shaped, as depicted above, a round, very strong mound of earth, with a small turreted top, the earth resting upon an arched structure, and within the vault, as it may be called, room for two lions, perhaps three. The gardener will tell us some strange and yet true stories about his master and the wild beasts and the tame beasts. He will relate that one evening, as the master came into the meadow by the large gates, there was a cry from the house, warning him that one of

the lions had broken loose, and was wandering about the meadow; and that master, taking no heed of danger, whipped his handkerchief out of his pocket, and marching boldly up to the lion, flipped the beast back into the den, and securely shut it in. The same servant will tell us of another incident. He will relate that one evening, there being a great noise and dog-barking heard in one of the yards, Mr. Hunter was called out of the house, to discover two leopards loosened from their chains, fiercely contesting with the dogs; that master rushed into the yard, and seizing each leopard by its collar, one with one hand, the other with the other, dragged the wild animals into their lairs, shut them up, and then, when the excitement had subsided, fainted clean away. He may further state, that one day he found his master wrestling with a young bull, which had been received as a present from the Queen, and that but for his (the gardener's) own fortunate intervention the master would have been gored to death. Perchance he might add one more story, rather gruesome in its way, and in these more refined days better left untold.

If now we cross the grounds from the lions'-den towards the right hand corner of the house and to an out-building there situated, we shall find, in a quiet recess, a copper, of which the annexed sketch is a faithful representation. The copper, we shall learn, is the cauldron in which the body of a giant, the Irish O'Brien, was boiled down, in order that the Hunterian Museum might have the skeleton. Possibly, the manner in which the skeleton was obtained will be told to us. How the young giant, being near his death from phthisis pulmonalis and alcohol, was aware that Mr. Hunter was after his body, and, to avoid that fate, left sufficient money to pay a posse of his countrymen to convey his remains to the

Nore, and cast them into the sea, at a sufficient distance to prevent them being reclaimed; how Hunter, after O'Brien's death, found out and bribed the undertaker with a round sum of £500; how that worthy official, accompanying the procession, led the way to an inn, where



The Copper as still standing in 1886.

the door being too small to admit the coffin containing the remains, he had the coffin carried to a barn, and deposited there for a brief interval of rest, after a weary walk on a sweltering day; how the barn door was carefully locked, and the key of it safely deposited in the pocket of the captain of the escort; how the bearers with their captain

and friends went into the inn to be right royally entertained by the kind undertaker; how, ere the entertainment had scarcely commenced, some men, concealed in the barn, removed the giant from his coffin, and exchanged him for paving stones of the same weight as his body, and hid the body in the straw with themselves; how the faithful but innocent Irishmen, after refreshing their inner men, were brought back by the undertaker to their bearers' task, and, never dreaming of the trick played upon them, marched on stage by stage, carrying their precious burden of stones towards the sea, into which they solemnly consigned it; how the undertaker, discovering he had forgotten something at the house where the exchange of stones for the giant had been effected, went back, got the body into a spring-cart, brought it to town, and in the night deposited it in the Museum at Castle Street; how Hunter carried it away from the Museum in his own chariot to Earl's Court; and how, there dismembering it, he boiled it down in the copper, and collecting the bones, much darkened by the haste and process of boiling, put them leisurely together into skeleton form, and held his peace on the subject for five years.

Finally, in the course of our visit at this country box, we shall be sure to see the bees that are undergoing observation; the hedgehogs, some of which have come from Edward Jenner, of Berkeley, in Gloucestershire, an old and very favourite pupil of the master; a considerable number of specimens preparing for transmission to the Museum; some living animals tended with great care, in order that they may yield good evidence as to the mode of growth of the long bones of their limbs; some deer, the antlers of which are under study; some cocks, whose combs have been frozen to death and restored to life; sundry skeletons of various animals in

course of fitting up for their place in the Museum; some curious instruments of research, including specially many newly constructed and valuable thermometers for determining animal heat and the ranges of it in different classes of animals; some arrangements for trying the process of incubation; some rare plants growing under close observation in respect to conditions of slow and rapid growth; and, in various positions, here and there, mounds under which, if we were to turn up the soil, we should find animal remains, especially bones counted as refuse, not good enough for the Museum.

In and amongst all these evidences of research and of untiring labour our philosopher, in the spring, summer, and autumn evenings, is at home. Company may occasionally drop in, for Mistress "Anny" could not live without company; but that interferes little with the musings of a mind which is its own kingdom, and which, knowing that its mortal days are sharply numbered, grows day by day more determinate to make the most of every fleeting hour.

Suppose, after this, we leave Earl's Court and attend the master to the hospital and the lecture-room. There we shall see him as the teacher, practical as well as theoretical. With manner and speech of him we shall not be entranced, for he is a bad expositor by speech, and does not attract more than twenty-five to thirty students at most. When he first began, it is said he, in gentle joke, begged the porter to let down the skeleton, that he might say "Gentlemen." In spite of his defects, in spite of his use of terms not always choicely selected, we find ourselves bound to listen to him, and feel, Dr. Beddoes notwithstanding, that we are in the presence of an extraordinary teacher, one who thinks for himself, and one who thinks beyond other men on all subjects which

he lays claim to as his own. What we are most struck with is his comprehensiveness; with him medicine is the same as all science connected with life. Animal heat is a single problem to be studied as much from a beehive as from the body of a man. Animal motion is all on one plan; there may be details of secondary powers, of leverage, and such-like, but the primary is the same in all: there is only one primary animal motor. Moreover, and above all, we discover in this strange man an earnestness, an acquisitiveness for natural objects, a grasp of understanding of natural objects which is phenomenal. He has no time for any study of men's devices. Not a moment! "They once tried to cram Latin and Greek into him; but he crushed their little plans like this,"—motioning an action which is better conceived than described,—"and they did not try it a second time." This, as a skit at scholarly brother William, is told with gusto, as if to indicate that William did not know John quite completely.

Such is our man as we, in description drawn from fact, may read him even now—John Hunter at home in London and in his country box at Earl's Court.* Let

* The descriptions I have drawn of places are from the places themselves. I have lately visited the house and Museum buildings in Leicester Square; and Earl's Court House—long the residence of my late friend, Dr. Gardner Hill, of non-restraint celebrity—was, for many years, as familiar to me as my own dwelling, with the lions'-den still in its grounds, the copper, and with relics of bones in the garden, a few of which, by the way, Dr. Hill gave to me, after they had been turned up during some alterations, instead of letting them go to the refuse cart. Mrs. Hunter's boudoir and drawing-room remained just as I have described them, up to the time when the house, itself little changed, was pulled down in 1886-7. Two of the anecdotes I have also from direct evidence; for in the year 1850 my most valued friend, the late Dr. Robert Willis, of Barnes, took me to Kensington to see the son of Mr. Hunter's gardener, a lad ten years of age at the time of Hunter's death, who remembered Mr. Hunter quite well, and

us now turn to this master, so as to trace him, in rapid sketch, from his birth onwards, through his various stages of development to the point where we have met with him during the height of his living fame.

BIRTH AND COURSE OF LIFE.

The life history of John Hunter is not one requiring a long story. He was born on February 14th, in the year 1728, at Long Calderwood, in the parish of Kilbride, East Lanarkshire. Kilbride is about eight miles from Glasgow, and I recall it, from my student days, as a place noted for its annual cattle shows, at one of which I remember visiting it in 1846, and trying, ineffectually, to make out where Long Calderwood was situated, for I had been doing some amateur carpentry in a shop in Glasgow, with the intention of making a new apparatus for reducing dislocation of the shoulder, and the owner of the shop had told me it was the "very place whar the famous John Hunter had tried his prentice han' at a wee bit o' cabinet work when he was a youngster." How I missed the house I cannot say, for it was a good stone-built Lanarkshire farm dwelling, as Sir William Ferguson's drawings of it, presented to the College of Surgeons in 1867, testify.

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was at Earl's Court at the moment when the encounter with the lion took place in the meadow. The account of the mode in which the Irish giant was removed to Earl's Court was told me by my old and illustrious friend, Sir Richard Owen, who had it from Mr. Clift; a fact I mention specially because the story differs from that of Mr. Drewry Ottley and other biographers. The sketches of Earl's Court House, of the lions'-den, and of the copper, were drawn for "NATURE," shortly before their demolition, by my son Bertram Richardson, and the sketch of the house in Leicester Square, as it now stands, is from the pencil of the same artist, engraved, together with the view of Earl's Court in Hunter's day, by my good friend Mr. Charles A. Ferrier, F.L.S.

The father of John Hunter was a small farmer, living on an estate which had come down to him through the Hunters of Hunsterston, from the time of Robert the Second of Scotland. His mother was the daughter of Mr. Paul, the treasurer of the city of Glasgow, and probably, from his name, of Semitic descent. There were ten children in the family, five of whom only, James, William, John, Dorothea, and Agnes, lived to grow up. Of these James, the eldest, took first to the law as a profession, but left it for physic, in which profession he promised to excel, but died of consumption before he had time to distinguish himself. William we have already had before us as the renowned founder of the Hunterian Museum, now at Glasgow. Dorothea married Professor James Baillie, and gave birth to the famous Dr. Baillie, author of the classical "Morbid Anatomy," and to the distinguished authoress Joanna Baillie. Agnes was married to a Mr. Buchanan, a cabinet maker in Glasgow; a happy-go-lucky sort of man, not a man of business habits, but convivial, unsuccessful, and short-lived.

John Hunter, ten years the junior of William, lost his father in the eleventh year of age, and from that time grew up a spoiled country boy, caring nothing for books, and wandering about the country side pretty much as he willed. I cannot find anything satisfactory about his going to school anywhere, but he must have got some rudiments of learning, for he wrote a plain hand, though he was not particularly good at the art of spelling. He was fond of country sports, and in them acquired a great deal that was of service to him in after life in relation to the study of plants and animals. When he was seventeen he went to Glasgow to live with his sister Agnes, Mrs. Buchanan, and it is said that he learned to assist her husband, the cabinet maker, whose negligence

was leading him to the wrong road of life. It is now that Hunter, as it is assumed, worked in the shop to which I referred a few passages above. He remained with Mr. Buchanan between two and three years, but as that gentleman's prospects did not improve, and probably because work, at a mere mechanical calling, did not suit the genius of a youth of John Hunter's tastes, he, John, determined upon going up to London to join his brother William, already making his way, by rapid strides, to distinction as a teacher of anatomy and practitioner of medicine. Some of John's admirers have expressed the view that their master had laid hold of his brother James's books on physic and surgical instruments left in Long Calderwood, and that in this way he was inspired into medicine. There is not a tittle of evidence on this subject, and the probabilities are far greater that a love of new scenery and of a new pursuit in a higher sphere of action, were the true incentives to the course pursued. Whatever might be the cause of the change, John Hunter, in his twentieth year, set out on horseback from Glasgow to London, took a fortnight over the journey, with a friend of the family, Mr. Hamilton, as his companion, and entering the metropolis in the middle of September—1748—was ready to commence his studies in the month of October at his brother's dissecting-room, under the direction of the favourite demonstrator and assistant, Mr. Symonds. According to custom—a custom still not dead—Hunter was first put to dissect the muscles of the upper extremity, a task he performed so dexterously that soon afterwards he was directed to dissect out the arteries of the upper extremity in an injected specimen. Again he succeeded so well that his critical brother foretold in him an anatomist who would never want employment.

During the following summer session John Hunter,

through the influence of his brother, was permitted to attend the hospital at Chelsea, under the instruction of the famous Cheselden, who, in a kind of retirement from the severer labours of practice, was surgeon to this Chelsea establishment. Here for two sessions—1749-1750—Hunter pursued his surgical studies with one of the most illustrious surgeons of his age. Unfortunately illness soon forced Cheselden to retire altogether, and on this event the pupil was transferred from Chelsea Hospital to Bartholomew's, to become surgeon's pupil to another surgeon little less famous, Percival Pott. He continued at Bartholomew's during the summer of 1751, and, occasionally, during the next winter; but in 1752 he returned to Scotland to fetch his sister, Mrs. Buchanan, by this time a widow, to London. A new stage in Hunter's progress now occurred. He went to Oxford, and entered as a Gentleman Commoner at St. Mary's Hall. Some mystery has been thrown about this matter, but really there is no occasion for any mystery. It is clear that classical brother William was at the bottom of it, found the money, and pressed the learning. John had lived with him, had sat at his bachelor's table where the accomplished and wealthy were also accustomed to sit, and the uncouth Scotch boy had shown that he did not shine as a scholar. William, acting on the idea that manners make the man, urged the rough youth to undergo a term of University life, and, perhaps, at the same time, hoped to remove him from a circle of acquaintances amongst whom he passed as "Jack" Hunter, and with whom he frequented the shilling galleries of the theatres to hoot and stamp and hiss at the players whenever an excitement was desired. The University plan, however, did not succeed; Hunter rebelled, and in a very unceremonious manner told Sir Anthony

Carlisle, in his later days, how he crushed the design. He was not going to have Latin and Greek thrust down his throat, not he! They wanted to make an old woman of him! But they didn't.

He came back to London, and entering as a pupil at St. George's Hospital, on the surgical side, in the year 1754, became attached to that institution, as it turned out, for the whole of his life. In 1756 he served as house surgeon to St. George's, and, still assisting his brother in the dissecting-room, learned practice in the wards of the hospital, and gained kudos also there.

At this period, when he was twenty-six years of age, a serious event took place in his career; he fell out with his brother William. The story of this quarrel was laid in jealousy—jealousy on both sides, and differently explained by each. John claimed that from observing an injected specimen of the veins and arteries of the uterus, made by Dr. Mackenzie, a pupil of Dr. Smellie, he discovered the mode of the connection between the placenta and the uterus, and communicated the discovery to his brother, who first laughed at the idea, and then adopted it as his own. William, on his side, claimed the discovery, or original observation, as entirely his own, and accepted his brother John's assistance as not more than the assistance rendered by a junior in whom he confided. The dispute, as we shall see, led to a correspondence between the brothers and the Royal Society, but neither gained a victory, and the rights of the disagreement remain still unsettled. In 1862 I heard two scholars quarrel upon the subject as ardently as the Hunters themselves might have done at the time when the angry feeling between them was at its height.

Notwithstanding this dispute, the brothers continued for a time to work together, and, in fact, John entered

into a kind of partnership with William in professorial labours. They laboured well, John with the scalpel, William with tongue and pen, weapons with which John was not good; fought the Edinburgh Monros on the discovery of the functions of the lymphatics; accused Pott of cribbing from them his views of the nature of congenital hernia; and made some new researches, in which John put forth opinions, that have not held water, against the veins acting as absorbents.

Work in the dissecting room and in experiment was continued by Hunter until 1759, when bad health compelled him to leave London and take rest, or, more correctly speaking, change, abroad. His brother James had died of phthisis, and it was feared that he might follow in the same way; he obtained, therefore, a staff-surgeon's appointment from Adair, the Inspector-General of Hospitals, and in 1761 went out under General Hodgson and Commodore Keppel to the famous siege of Belleisle, on the western coast of France. After this event, he accompanied the troops to Portugal and Spain, during which time, though engaged in campaigning, he managed to commence some studies in natural history, in order to determine if digestion goes on in snakes and lizards during the months when they lie in the torpid condition. Of his character as a staff-surgeon we have no information, except from a rival cotemporary, one Jesse Foot, who speaks of him as contentious, and as delighting in exciting jealousies amongst colleagues. We have seen that Hunter was fond of making an occasional disturbance at the theatre, and he may have excited a little quarrel now and then, just sufficient to have afforded Foot some pretext for his colourings; but as Foot was anxious to be reckoned as a rival of Hunter in after life, it is essential to take all he has said with

the customary grain of salt, and without any sense of surprise that Hunter made no reply to his denunciations.

In 1763 Hunter returned to London, the war having ended, and at once settled himself down to work on his own account. It is probable that he would have taken this independent course even if he had had the opportunity of rejoining his brother; but that opportunity was not offered him, for, during his absence, his place had been filled up by William Hewson, whose name is still familiar with medical scholars. Hewson was a fine teacher, a hard-working and conscientious anatomist, an original explorer, and one in whom William Hunter had every confidence. So it happened that in 1764 the brothers Hunter had two schools, William's school in Jermyn Street, and John's in his own house not far away,—perchance in the same street. The school of John consisted of pupils whom he took to live with him, and from whom he received five hundred guineas as an apprenticeship fee; but very few others joined his class, for he was a poor lecturer, and never won the popular ear either in or out of the profession. In like manner he was for a long time unfortunate in practice. He laboured under many disqualifications for practice. He was rough in manner, and in speech delivered himself in a style that was unpleasant to ears polite. The old name of "Jack" Hunter stuck to him pertinaciously, while conscious of his genius he carried himself amongst his richer and more fortunate compeers with what Drewry Ottley calls that "pride of port," a pride which is often keenly offensive, and which makes even amiable men of genius, to say nothing of rough ones like Hunter, objectionable to those with whom they have to associate. In this there is nothing wonderful, for genius has, of necessity, its own way of dealing with a world over which, though

it lie at its feet, it may have no immediate control, and from which it may receive no immediate recognition.

It is only just to say of Hunter that, with all his roughness, he carried a pure independence; he never truckled for advancement, never sought favours, and was at all times more ready to give than to receive. He knew no tricks of trade in professional work. If patients came to him he gave them his best attention, and there the matter ended; he was not obliged to them for coming, and he laid no snare to bring them back again. Such a man must needs wait for practice long and wearily, and may consider himself fortunate if he get any at all. Hunter did wait, and, after a time, practice came to him to the tune of about six thousand guineas a year; a goodly income, but one which, in his case, went to the advancement of science, to the development of medicine, and to the foundation of the incomparable treasures which now lie stored in the Royal College of Surgeons in Lincoln's Inn Fields.

Very soon after he had settled down into practice he bought or took the "Box" at Earl's Court as a convenient place for study, and a kind of feeding-house for the Museum; he obtained permission to have the refusal of all the show animals dying in the Tower, and he made free use of opportunities to possess the animals which, in those days, were carried round the country for the fairs and feasts as favourite shows. He conceived now the grand idea of comprehending the whole kingdom of life; of contrasting and comparing one field of living things with the whole, and the parts of the whole with each other, so as to make nature his sole and indisputable mistress. His was to be a classical school, the classic of the classic in natural history, with no authority, save the one authority that has framed all, and planned all,

and given to all construction and form. The grandeur of his project was not slow in its effects; Hunter the junior got more recognition than Hunter the senior amongst the leaders of the world of science, and in 1767 he was elected a Fellow of the Royal Society, an honour for which William had still to wait. He was fortunate, too, in being elected, on December 9th, 1768, Surgeon to St. George's by a majority of one hundred and fourteen votes over forty-two obtained by his rival, Mr. D. Bayford.

In 1770 William Hunter removed his school and Museum to Windmill Street, making over the lease of his old house and school in Jermyn Street to John—another advantage. For the new school John acquired some pupils, who afterwards attained wide-known names, including, amongst others, Edward Jenner, for whom he seemed to have entertained a fatherly kindness, in which affection with a fair amount of chiding and occasional snubbing blended curiously. The year following his possession of the house in Jermyn Street, namely, in July 1771, Hunter married the beautiful and accomplished Anne Home. He was married not altogether according to bachelor brother William's humour; for William had an eye to the one thing needful, and a wife who had no dowry was, to his mind, a pure mistake for a poor man, however great might be her beauty, and her ability to lead and shine in society. But John had good taste; he was by nature an artist, and loved pictures above everything; how then could he fail to desire that the choicest picture in his home should be the most lovely? He was earning at this time about a thousand guineas a year—too little for a man to found a Museum upon and at the same time marry a wife ambitious to excel in the world of fashion. So said such friends as brother William and those of his circle.

Some years passed, during which Hunter was busily engaged in adding first to his Museum and his learning of nature, and next to his practice as a surgeon. In all he succeeded, and up to the year 1775 everything was doing well; but in the spring of this last-named year, after some mental excitement, a check of a smart kind interrupted the course of his life. He was seized suddenly with what was called spasm of the heart, during which he contrived to breathe by a voluntary effort, though, as he afterwards observed, this was probably of no service because the circulation had stopped. After a most painful paroxysm he made a quick recovery, and from then onwards, though the paroxysm was a warning, had a long respite. In 1777 he had an attack of another kind, which he described in a letter to Jenner as "a swimming of his head so that he could not raise it from the pillow for ten days," with other symptoms so serious, that on the advice of his medical friends, including the distinguished Heberden, he retired to Bath for three months, leaving Everard Home (Mrs. Hunter's brother) and Mr. Bell to make or complete a catalogue of the specimens in the Museum. This last attack was also brought on by mental worry. He had become security for a friend for a large sum of money, and had been called upon to pay the liability at an inconvenient moment. His symptoms were, inability to raise his head from the pillow; morbid sensibility of the organs of sense; a feeling of being suspended in the air; a sensation that his head and body were diminished in size; and an idea that every motion of his body was extensive and rapid. By the time the three months' rest in Bath were over he was tired of the enforced idleness, and returning to London, recommenced his labours with improvement in health and apparently a fair recovery. In fact, he

resumed his lectures, including surgical lectures at St. George's, and worked away in capital spirits as if work were play, as a letter to Jenner indicates.

Jenner, poor fellow! always soft-hearted, had been crossed in love, and had poured out his soul to the irrepressible "dear man" John Hunter. The "dear man" replied with characteristic bluntness, but not in the best grammar: "I own I was glad when I heard you was married to a woman of fortune; but let her go, never mind her. I shall employ you with hedgehogs, for I do not know how far I may trust mine." This was quite in character with Hunter when he was in strength, and shows that he had got back to his old power. The same evidence is repeated in an act which even his staunch adherents somewhat regretted; he sent a paper to the Royal Society in 1780, renewing his claim of having discovered the true mode of union between the uterus and the placenta, the discovery his brother had already claimed and had published in the year 1775 as his own, in his great work on the gravid uterus. It was a pity; and, worse than all, the defence of John to the Royal Society, in answer to a dignified letter by his brother, was just wanting in that dignity which his brother had sustained; for in his letter he suggested that if he himself did not make the supposed discovery it ought to be divided amongst a number of others. Of course the Royal Society did not permit itself to enter into a controversy between two of its Fellows on a matter entirely of personal veracity, and so the public interest in the question subsided. But the quarrel between the two chief disputants was irreconcilable, and although, a year or two later, in the latter part of the fatal illness of William Hunter, John begged to see him, and did, for a short time, lend him his

professional aid, there was no true reconciliation, and the splendid collection which the elder brother had made, and which would have been a fortune to the younger, was left away from him, together with all money and even the family estate at Long Calderwood. That John Hunter smarted severely under this sad trial will be obvious enough to every one who thinks of the strong emotional nature of the man. Baillie, to whom the little family patrimony was bequeathed, in a truly generous spirit, restored it to his aggrieved relative; but that was not sufficient to heal a moral wound, to some extent self-inflicted, that pierced the heart.

A diversion of a serious kind, in the year 1781, led the thoughts of John Hunter away from the dispute with his brother, and caused his name to be spoken of amongst the public more than it had ever been previously. He appeared as a witness for the defence in the remarkable trial, at Warwick, of Captain Donellan, for the supposed murder of Sir Theodosius Boughton. Hunter was on the unpopular side, was sneered at by the judge, and was practically of no service to the unfortunate prisoner, who, tried on the suspicion of killing his brother-in-law, Sir Theodosius, by means of laurel water, was condemned, verbally, by a kind of legal sophistry, for killing by arsenic. In the Medical Society of London we have a report in full of this trial, and reading it now we see that of scientific evidence, so called, there was none worth consideration compared with that given by Hunter; yet his evidence went for nothing, and Donellan, "poor devil," was executed.

From 1782 the tide of practice began to flow towards Hunter, and with it came extended fame. He was elected a Fellow of the Royal Society of Belles Lettres of Gottenburg; Croonian Lecturer of our own Royal Society; and Member of the Royal Society of Medicine and of the

Royal Academy of Surgery of Paris. We may consider him now in fact as in the zenith of his reputation. He was also by this time a Member of the Corporation of Surgeons of London, but singularly enough seems to have troubled himself little with their affairs, and held no important office in their fraternity.

In 1783 Hunter made his great move from Jermyn Street into the house in Leicester Square, of which we have already heard. Here he was indeed at home, and here he gave a local habitation and a name to the "Lyceum Medicum" and to the "Society for the Improvement of Medical and Chirurgical Knowledge," the Transactions of which are still a treasury of useful facts and observations. The Museum, under the careful hands of Everard Home, Andre, and Bell, became also gradually complete, and by 1785 may be considered to have attained perfect order. In this year he was honoured by the award of the Copley Medal from the Royal Society, and in the early part of the year was in good health; but in April, after some symptoms of gout, he began to suffer from spasmodic seizures of the arms, face, and stomach, and subsequently with what was again supposed to be spasms of the heart. He was also attacked with nervous oppressions, and with a dread that he would be the victim of hydrophobia, because he had made a dissection of a person who had died of that disease. His severe attacks of spasm were easily induced, and ordinary remedies failing, he was led to visit Tunbridge Wells and afterwards Bath, from which latter place, having regained much of his usual health and spirits, he returned to London in October, and resumed his active work.

Three more incidents, and this section of the history of our great man must cease. He stood as sponsor to one of Jenner's children, a responsibility accepted by

him in a letter truly characteristic;* he took part in the formation of the Veterinary College; and he sat for his portrait to Sir Joshua Reynolds.

The original portrait from this master engraved by Sharp, and destined to become one of the household gods of physic, is in the Royal College of Surgeons; but there was another taken by the same master, which Mrs. Hunter presented to Mr. J. Weatherall, from whom it came into the possession of Mr. Thomas Knight, a member of the Society of Apothecaries, who in turn presented it to that Society. Through the kindness of the present Master of the Company, Edwin Chabot, Esq., and the Court, I have been permitted to have an autotype taken of their portrait, as the one which to my mind is the truest because the least conventional. In this portrait we have the man in his work-a-day form; not abstract, but real; not thinking of what he ought to be, but what he really was; in illustration of his own idea that no man is great who tries to be so.†

Two offices were bestowed upon Hunter in his later

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* See ASCLEPIAD, vol. vi., No. 23, p. 279, 1889.

† There are other likenesses of Hunter; a profile from the life by Tossie, engraved by J. Caldwell; two oil portraits by Edward Home, Mrs. Hunter's brother, in one of which he is shown at work in a loose coat with his hair brushed back; and, in another, now in the Royal Society, with his hand on the head of a dog. The Royal College of Surgeons has a bust of him by Flaxman, a small medallion, and a fine engraving produced by Maguire, in 1849, and painted by Hanhart, from a pencil-drawing taken in 1793 by Sir Nathaniel Holland. Palmer published also a silhouette, and the late Mr. John Churchill picked up a plate of him by J. Kennerley. From the original portraits of Hunter many plates, busts, and statues have been produced since his time; including the statue by Weekes in the Hunterian Museum, and the bust in Leicester Square, by the sculptor-poet, or poet-sculptor—which shall it be in future history?—Thomas Woolner.

years. In 1776 he was appointed Surgeon Extraordinary to the King, and ten years later he was made Deputy Surgeon General to the Army; but, on the whole, his position was not great amongst the courtly people. His practice must, I think, have been general as well as surgical, including midwifery; for Mr. John Cordy Jeaffreson has found conclusive evidence that Hunter attended Mrs. Byron at her accouchement, when her babe, afterwards the famous poet Lord Byron, was born in Holles Street, in January 1788; that a short time afterwards he gave Mrs. Byron instructions respecting the kind of shoe her child should wear on coming to need shoes; and that, in May 1791 he was consulted on the subject of the proper shoe for the well-known deformed foot with which the poet was afflicted from birth.

JOHN HUNTER AS A WORKER.

From the above outlines of the career of Hunter so far, let us turn to him briefly as a worker—to his work as an author, as an original observer, and as a practitioner.

WORK AS AN AUTHOR.

In order to arrive at an estimate of the writings of John Hunter, I have made it a point, as a student preparing for the task, to re-read carefully the Hunterian essays, through and through, from Palmer's admirable edition of 1835. Palmer was a conscientious and laborious Hunterian scholar, an erudite man, and one blessed with true common sense, by which he was guided in the sterling foot-notes he has added to his volumes.

Hunter first appeared, as the author of a comprehensive work, comparatively late in life, namely, in 1771, when he was forty-three years old. In this year he published his first volume, "*A Treatise on the Teeth;*" a treatise, it has

been said, that was forced out of him in order to defray, from the sale of it the expenses of his impending marriage In 1772 he published his essay on "*The Digestion of the Stomach after Death;*" in 1773, "*Anatomical Observations on the Torpedo;*" in 1774, "*Account of Some Receptacles of Air in Birds;*" in 1775, "*An Account of the Gymnotus Electricus;*" in 1776, "*Proposal for the Recovery of Persons Apparently Drowned;*" in 1780, "*Account of a Woman who had the Small-Pox during Pregnancy, and who seemed to have Communicated the Same Disease to the Fœtus;*" from 1776 to 1782, *The Croonian Lectures on Muscular Action*, for which and other papers he was awarded the Copley Medal in 1787; in 1784, "*Observations on the Inflammation of the Internal Coats of Veins;*" in 1786, "*Treatise on Venereal Disease;*" in the same year, "*Observations on Certain Parts of the Animal Economy.*" In 1792 he wrote "*A Treatise on the Blood, Inflammation, and Gun-Shot Wounds,*" a work dedicated to the King. This was the last, and some think the greatest, of all the Hunterian works. It is certainly the best written, although in places obscure both in thought and expression. It was not published until the year after his death.

There are many other medical essays appearing casually, such as one on *Introsusception;* another on *Paralysis of the Muscles of Deglutition Cured by an Artificial Method of Conveying Food and Medicines into the Stomach;* a third on *The Case of a Young Woman who Poisoned Herself in the First Month of Pregnancy;* and a fourth *On the Growth of Bones.* There is also a paper by Everard Home in the Transactions of the Society for the Improvement of Medical and Chirurgical Knowledge, describing Mr. Hunter's *Operation for the Cure of Popliteal Aneurism*, and which really belongs to the Hunterian series.

In the above list all essays and papers that relate to natural history, as distinct from medicine and surgery, are omitted; but those mentioned have been of sufficient weight for many months of study. I rise from that study, as I am sure every other student would, astounded at the quality, the magnitude, and the grasp of the author. The works are phenomenal. In them are texts for Hunterian orators for a thousand years at least. They stand for originality second to none in medicine, and although they do not contain any one concrete discovery like that of the circulation of the blood, they have in them much more that partakes of comprehension of nature in life and living action, and in compass of vital, natural law.

In the study of Hunter's writings the difficulty of completely understanding him is often perplexing. He observed so many things, held such breadth of view, and tried to compress so much into so little, that had he possessed natural or acquired skill in exposition he would have been hard to follow. It is perfectly honest to say, that easily as he gathered and treasured from nature, he was, according to ordinary ideas, an illiterate man. He composed chiefly by dictation, and with little more appreciation of the learning of other men than he would pick up from casual conversations. He read from nature, and took no time to read from any other source. Here was his strength, here his weakness. He was in and within nature. He was in collusion with her, and if he could have expounded her with half the facility that he could read her, Aristotle himself had been his inferior. As it was, he was like a man who has learned the language of a foreign country by living within it and associating with its people, without being able to translate the language

into his native tongue. At times, in his admiration of natural acts and processes, he mistook nature by assigning to her intentions, as if she went out of her way to do some particular thing in order to meet some particular accident or event. In this sense he speaks about healing "by the first intention," as if nature were a surgeon called in to meet a special injury by a special design of art. He even attributed good to her when in adhesive processes she led to the most fatal injuries.

From the fertility of his mind he must, like Shakespeare, have composed with rapidity, straight off, without either blotting out, referring back, or correcting carefully; from which circumstance he often contradicts himself. Thus in his first Croonian Lecture on muscular motion he tells us that when voluntary muscles take on involuntary action they never tire. But a page or two further on he explains that muscles after work require a certain time to regain the power they have lost in action, and that by alternate motion, quick and violent, they may tire sooner than by constant action.

It is most difficult to say which of his works and observations is the best, for there abounds in every chapter such brightness of originality, that one great sentence will sometimes compensate for any number of obscure or doubtful passages. The Croonian Lectures; the essays in the "Animal Economy;" Chapter II., on the "Vascular System," in the work on the blood; and the essay on "Sympathy," in the "Principles of Surgery," are, to my view, the finest. The most unsatisfactory of his works is that on the Lues Venerea, for here he has confounded syphilis with gonorrhœa, appears to consider all chancres as indura-

tions, and treats upon mercury as if it were an absolute specific, and as if, by its action, it would diagnose between the specific and a non-specific affection. In criticising this treatise it is, however, necessary to remember two facts: first, that the work was supervised by four other practitioners, namely, Blane, Fordyce, Pitcairn, and Marshall, so that he was not so absolutely responsible for it as for his other works. In the second place it must be admitted that some of the most accomplished men of our own time—take Dr. Wilks in illustration—have defended Hunter with masterly argument. Dr. Wilks' essay, "A DEFENCE OF HUNTER," —if I remember correctly a title of nearly a quarter of a century ago,—was a splendid effort. It commended the opening chapters of the Hunterian work; accredited Hunter with taking a correct view of syphilis as a specific constitutional disease; excused, cleverly, his apparent ignorance of the difference betwixt syphilis and gonorrhœa; explained that Hunter anticipated Rollet on the question of the "*Chancre Mixte*," and defended the Hunterian treatment, contending that in that treatment two factors were proposed: (*a*) the radical cure, in an early stage, by destroying the chancre; and (*b*) the constitutional cure after the disease has itself infected the constitution.*

WORK AS AN ORIGINAL OBSERVER.

As a patient and original investigator John Hunter was one of the most laborious and comprehensive

* It would be very useful if Dr. Wilks would republish his essay on the defence of Hunter. Few beyond myself probably remember it now, and my recollection of it is imperfect; but I hope that in the few lines submitted I have correctly stated those parts of the argument to which reference is made.

the world has ever possessed. He was always on observation. In the year 1767 he ruptured his *tendo Achillis* whilst dancing, and, wishing to test treatment, he continued to walk about with the heel raised and the muscle simply compressed with a bandage. By no voluntary act could he induce the muscles to contract when their tendon was divided, "the muscles refusing to act as if from a sense of imperfection." Afterwards he traced out the process of reunion of divided tendons in the lower animals, dividing them, artificially below the skin, with a couching needle. In the remarkable chapter on "SYMPATHY," in the *Principles of Surgery*, he tells us he went to an animal magnetizer to be magnetized, rather fearing that his imagination when acted upon would lead to spasm of his vital organs; but he remembered that he could direct his attention to any part until he felt a pain in that part, and so when the magnetizer told him he would feel the magnetism first at the root of the nails of his hand nearest to the so-called "magnetic" apparatus, he directed his attention exclusively to his great toe, working it about, and preventing the "magnetism" having any effect upon him.

In regard to experimental work it must be admitted that many of his experiments were rough, and that some very inconclusive ones led to incorrect inferences, as, for example, in his argument that the veins do not absorb; but when he came to anatomy and to the broad outlines of natural history; when he came to comprehensive grasp of vital phenomena, then he was equal to the best of his race. Geology (Palæontology, as it has been called since his time); Botany; Natural History; Anatomy, comparative as well as human; in

all these his genius and grasp of observation could not be surpassed.

WORK AS A PRACTITIONER.

The most varied opinions have been held respecting Hunter as a practitioner of medicine and surgery. Some of his cotemporaries, following the example of those of Harvey, derided his practice, and looked upon him merely as a naturalist and enthusiastic physiologist. Others, including Astley Cooper, extolled his practical skill. The plain truth is, that his mind was rather turned from practice because that was work for a living, and took him away from the original pursuits in which his soul delighted. I confess I should not like to be subjected to the practices he carried out, and to the bleedings and dosings he thought necessary. But here we must judge him by his time, and, so judged, he stands out favourably. He often suggested common-sense rules of life, rules sound to this day. He advised Nicol, the publisher, with whom he once went halves in the purchase of a fine tiger, that if he wanted to raise a young child and not kill it, he must not try to harden it, but must bring it up with the same care for its warmth as the hen bestows on her callow brood; advice acted upon with the success it deserved. As a surgeon he was a careful if not a brilliant operator; a surgeon who looked upon an operation as the opprobrium of surgery, but who did his work well when called on by necessity. Even in regard to the abstraction of blood he was more guarded than his fellows. His diagnosis was good for its day, and his observation of phenomena of disease acute. He was bent on observing the effects of one disease on the course of another, and pointed out the fact that he

even had seen *Noli me tangere* cured by the action of a fever. At the same time he missed things sometimes in a manner incomprehensible, except on the theory that his mind was too absorbed in physiological work to be quick in seeing the practical. How else can we account for the fact that he divided the tendons of animals with a couching needle, and yet never thought of subcutaneous surgery?

The great support of his claims as a surgeon is that he suggested and practised the operation of tying an artery for aneurism at a point far above the aneurismal sac. Had he originated this operation it would have been proper to place him in the first rank as an operator. We can, however, in presence of the truth, say no more of his help in this matter than that his explanation of the reason for the operation and for its success was strictly scientific, and confirmatory of a great advance in practical surgery. In strict fact, the French surgeon, Anel, had conceived and performed the operation before John Hunter ever took it in hand. I know it is the habit of our good Gallic brethren to give us wretched islanders little credit for originality; but this should not lead us into the temptation of making our great men greater than theirs, when the evidence is against us.

In summary, John Hunter was, by comparison, a superior practitioner of medical and surgical art; but in this work of his life he did not shine so illustriously as to stand out a burning and inextinguishable light, such as he does stand out in the physiological and natural history departments of natural knowledge.

DEATH, BURIAL, AND RE-BURIAL.

The story has often been told of Hunter's death.

On October 16th, 1793, he left his house in Leicester Square in good spirits, whistling, as Mr. Clift observed, a Scottish air. He forgot his engagement list, but Clift followed him to York Street, St. James's, where the carriage was waiting. He took the list from Clift, and in an excitable tone told the coachman to drive to St. George's Hospital, where he went to meet a Board, to insist on the admission of two young students to the hospital course, who had not fulfilled a new rule of proving that they had received a regular medical training. He had previously sent round a rather rude letter on this matter, and soon after the opening of the Board meeting, and whilst he was speaking, one of the Board flatly contradicted him. Trying to suppress his uncontrollable passion, he struggled into an adjoining room, his nephew, Dr. Baillie, after him, and, falling into the arms of Dr. Robertson, died on the spot. His body was put into a chair, and, with the empty carriage following, was borne to Leicester Square.

On October 22nd, 1793, John Hunter was interred in the vaults of the Church of St. Martin's-in-the-Fields, after a *post mortem* had been made, which revealed extensive heart disease and other lesions. He had expressed a wish that his heart might be preserved, but this was neglected. A very few friends attended the obsequies. After lying in vault No. 3 under St. Martin's Church or fifty-six years the remains were removed. An Order in Council had been advertised that the vaults of the church were to be cleared. The late Frank Buckland, whom we all knew and loved as the dearest and most genial of men, read this advertisement, and, with Professor Quekett, obtained permission to see vault No. 3 opened and emptied.

At the bottom of a number of coffins they found that of Hunter, in excellent preservation. The coat-of-arms on the brass plate was uninjured, and the inscription—

JOHN HUNTER,
ESQ.,
DIED OCTOBER 16TH,
1796.
AGED 64 YEARS,

was as distinct as on the day it was engraved. In the parish registry the following was also recorded:

October 22nd, 1793.
Apoplexy, M. John Hunter, Esqr., £6 10s. 2d.
Leicester Square. No Candles. ¼ past 4 o'clock.
V. N. 3. Duty 3d.

A wish was at once expressed that the remains should be taken to Westminster Abbey, and be there re-interred. To this the Dean of Westminster, Dean Stanley, gave his assent, and on Monday, March 28th, 1859, the remains were removed from St. Martin's-in-the-Fields to the Abbey, and were buried there immediately after the afternoon service. I was one of those present, and in the evening of that day wrote, for publication, the following account:—

"As public notice had been given that the re-interment of John Hunter would take place in Westminster Abbey on Monday after the afternoon service, and that an appropriate anthem would be given, an unusually large congregation assembled, and great numbers of medical men attended in addition to those who had obtained tickets at the College. There was no funeral service, but the words of the anthem during the afternoon services were, 'When the ear heard him, then it blessed him; when the eye saw him, it gave witness to him. He delivered the poor that crieth, the fatherless, and him that hath none to help

him. His body is buried in peace; his name liveth evermore.' While the service was proceeding the Council of the College and many gentlemen invited to join in the ceremony assembled in the Jerusalem Chamber, the room in which Henry IV. died, after having been brought there from the Confessor's Shrine in the Abbey, in a fit of apoplexy; and, the afternoon service over, the procession was thus arranged. Following the coffin, which was carried on a high bier, were the Dean of Westminster; Mr. Baillie, a grand-nephew of Hunter; the Earl of Ducie, and Dr. Clarke of Cambridge, as Trustees of the Hunterian Museum; Mr. Frank Buckland and Professor Owen; the Presidents of the Colleges of Physicians and Surgeons; the Council and Professors of the College of Surgeons; the Censors of the College of Physicians; the Master and Warden of the Apothecaries' Company; the Presidents of several learned Societies; the Medical Officers of the London and Provincial Hospitals, and many visitors. While the 'Dead March' in *Saul* resounded from the organ the procession proceeded round the Abbey through lines of spectators, and returned to a grave opened on the north side of the nave, near the western end; here the coffin was lowered amid a great concourse, and many present obtained then their first glimpse of it. The coffin was extremely well preserved, the grey cloth worn only in a few places. On a brass plate, with the family arms, was inscribed, '*John Hunter, Esq. Died* 16*th of October*, 1793. *Aged* 64 *years.*' Beneath this plate the College had had another affixed, with the inscription, '*These Remains were removed from the Church of St. Martin's-in-the-Fields by the Royal College of Surgeons of England, March* 21*st*, 1859.' "

In opening the grave for Hunter the bones of Ben Jonson were exposed, and a skull was freely handed about, said to be that of "Rare Ben;" but thereby hangs a tale, which some other day I will relate.

In April 1862 the Royal College of Surgeons completed their grateful duty by placing in the nave of Westminster Abbey a memorial of polished granite, inlaid and bordered with brass, bearing the words:— "*Beneath are deposited the remains of John Hunter, born at Long Calderwood, Lanarkshire, N.B., on February* 14*th*, 1728; *died in London on October* 10*th*, 1793. *His remains were removed from the Church of St. Martin's-in-the-Fields to this Abbey on March* 28*th*, 1859. *The Royal College of Surgeons of England have placed this tablet over the grave of Hunter to record their admiration of his genius as a gifted interpreter of the Divine power and wisdom that works in the laws of organic life, and their grateful veneration for his services to mankind as the Father of scientific surgery. 'O Lord, how manifold are Thy works; in wisdom hast Thou made them all.'*"

So rests finally this bright representative of the great lights of the last century of mighty genius; a century that has been a bank of knowledge and wisdom, on which we of this century have lived and flourished. In our money-grubbing, fee-gulping, pedantic, hard matter-of-fact days, with their hide-bound educational formulas, he would have been nowhere. The very College where he is worshipped would pluck him like a shot, as one fit only to be sent back, old chairs to mend. Happy for him and for us that, working in freedom from the tyranny of much cramming, his immortal mind was permitted to run, without impediment, its immortal course.

DEVOLUTION IN MEDICINE: THE TREATMENT OF DISEASE BY INFECTION.

ON passing out of the Royal College of Physicians this day, Thursday, January 15th, 1891, a man running with a bundle of newspapers under his arm was calling out his principal evening ware: "Disclosure of the Great Medical Secret! Special Edition!" Walking homewards, I found at the corners of the great thoroughfares the affiches spread out upon the ground, with wondering crowds reading the one chief event, told in different terms, but all with the same purport—"Disclosure of the Great Medical Secret." Shades of the Royal College! I thought, what would be said and done by you, if it were your fate to come to-night out of those doors and hear and see such an announcement? Harvey would surely have drawn the little dagger he was accustomed to carry, and been more demonstrative with it than usual; Arbuthnott,

"Who knew his art, but not his trade,"

would have wept; Halford would have walked by in scorn; Paris would have stormed; and Watson would have expressed the firmest and gentlest of regrets.

A secret in medicine, in legitimate medicine so called!

What food for disdainfullest, saddest satire! What food for men of the Tolstoi school, who look upon all representatives of medicine as "knaves, cheats, and scoundrels"! A secret in medicine divulged!—as if medicine were not feeble enough without enfeebling herself the more by making and then divulging secrets and mysteries. I have in hand a history of the science and art of medicine of the nineteenth century, that medicine and its professors. Hitherto, barring the labour which calls for much industry, the task has presented fewer difficulties than might have been expected; but this present chapter of it, how is it to be met? By what concord shall this secret be connected with the past abhorrence of the word and the deed? A great secret in medicine: the qualified and accepted brotherhood of healers acknowledging a secret, a secret means of cure!

It is not meet for physicians to find fault with one another. We are all, like poor Hamlet, indifferent honest, yet have we more offences at our back than we have thoughts to put them in, or imagination to give them shape. Ought we not, therefore, to be first to exercise that precious gift which suffereth long and is kind; thinketh no evil, doeth no evil, rejoiceth not in iniquity, but rejoiceth in the truth? We of all men. We ought, certainly, and yet we must not be servile. We must, with all conscientiousness, speak what we believe to be true.

When the last number of this work was published the medical world was in the height of *delirium agitans*. It was in a kind of medical hysteria, with the globus as a characteristic sign. In the half-century I have seen of physic nothing has approached it. The introduction of the grand blessing of anæsthesia was a trifle to it. The world was bewildered with the mere view of the excite-

ment. On one side it seemed clear enough that the wild statements and wilder prophecies were of the impossible; on the other side, men of sane reasoning power were seen running after a new prepossession like emigrants after a gold or diamond find; or, better simile, like the bands who might flock to the standard of a second Peter the Hermit. Surely there must be something in such a diversion from the ordinary courses of medical life; the multitude must be right, and in the multitude there must be safety.

Out of respect to this singular demonstration, I wrote guardedly. I guessed what the secret was they went out to see, and I ventured to indicate that the evidence of the great results expected could not, at best, be collected for years to come. But hope was expressed that every unhappy foreboding would be falsified, and that three months' testing would feed the more sanguine with some satisfaction and sign of bright expectation.

The fates have not been propitious. The secret is, partly, out, but many believers in it, whilst it was a secret, shrug their shoulders now and think without utterance. Ah, if they had but known that the remedy was a poison, administered in infinitesimal proportions, they would have left it for the homœopaths to manipulate, according to their dogma and their heresy! And here are the homœopaths laughing actually at us of the school of legitimate physic, because we have been caught vulgarly swallowing their dogma, admitting even the effect of the infinitesimal dose, and they themselves keeping out of all danger within their own lines. Incredible humiliation!

If we had but known! Yes, that is the weighty charge. If we had but known the secret! For the first time in the history of regular medicine a secret. On what

grounds shall a physician impose on his brethren a secret remedy, and remain worthy of the name of brother? There's the rub. Hitherto, the honourable physician or surgeon who has brought forward some new remedy has told the nature of the remedy to the minutest detail; why he used it in the first instance, and why he ventured to recommend it. Then others of his brethren have taken up the research, if, after understanding it, they approved of it, and quietly and unobtrusively have put it to the test. Finally, they, in turn, have related their experience, and, like light communicated from one torch to another, the practice thus initiated, if sound practice, has become so general that under the silent diffusion of it, its originator, without whom the whole thing might never have seen the light, may have become altogether forgotten; like the man who first used opium, or, passing down to our own age, the man who first used iodide of potassium. And, is not this the correct plan, the honest plan? Trace out the consequence, if *no* be the reply. Admit secrecy in the use of a medicine, and where is secrecy to end? By what standard shall that man be measured who shall bid us practise in matters of life and death on his secret? Be sure at once that every man of great mind would resent, as the grossest insult, the suggestion of asking for such a privilege. Think of William Harvey, or John Arbuthnott, or Thomas Young, or Baron Haller, or Xavier Bichat, or Sir Astley Cooper, or Professor Gross, or Sir Thomas Watson keeping a medicine or a remedial measure a secret. It is absolutely impossible to imagine such a thing. How could they be leaders of a practice which, once introduced into medical art, could be made to conceal the worst of crimes? A secret poisoner might thank his master, the devil, for the gift; but the men of

physic, pure and undefiled, in the face of the Eternal Power to whom all hearts are open and from whom no secrets are hid, would tell us that, whatever may be said, permissively, or even advisedly, in favour of secrets in other things, secret medicinal agents are simply out of the range, not of professional etiquette simply, but of professional morality.

The secret was the first and greatest mistake, but a second mistake was the fury with which the secret was followed medically. It does not sound or look well to the shrewd men of the world—or, for matter of that, the shrewd women either—for hundreds of medical men to troop off to witness a medical miracle. We perform miracles every day. To restore sight by removing a cataractous lens is a miracle. To amputate the tongue and prove that the man with his tongue cut out can still speak is a miracle. To obliterate sensibility whilst the surgeon cuts away a portion of the body is a miracle. These miracles came to us without any insane running after them, and why not others of the same importance? The facts are as strange as they are inscrutable. What could have led sober and staid men to believe that a disease like pulmonary consumption could be cured from the results of one set of laboratory experiments is simply impossible to say, and the more impossible now that the facts are better known. Couple with this rush and excitement the wild demands for the "lymph," the collection of the afflicted from all sources to exhibit them under experiment with a remedy the nature and action of which were unknown; couple with these the mortality and the violence of symptoms springing from the undeclared nostrum; add to all the miseries of the unhappy sufferers who, misled largely by false medical enthusiasm, left the known, if poor, paths of medicine by which they

could be conducted towards recovery, or failing that towards euthanasia, to have their means reduced, their fond hopes dispelled, and, in some instances, their short days shortened, and who shall calculate all the mischief that has been accomplished in this one paroxysm of confused judgment and hasty generalization?

Apology is claimed by some for all this derangement, that the remedy is a guide in the diagnosis of tubercular disease. Would that such an apology had been writ in the water in which it was conceived, and no more. There is nothing in which the accomplished physician is more accomplished than in the detection of tuberculosis. Even a student who could not at his "final" make the diagnosis would stand a good chance of being plucked. But suppose we wanted some new mode, would any sane physician, who had the welfare of his patient at heart, subject a patient to a kind of modified snake-bite in order to settle a refined point of diagnosis, which really would not affect his judgment as to the best treatment to be pursued one iota?

That the results of the secret remedy should have been what they are is no wonder now that the secret is broken. The effects are precisely those that would spring from the injection into the living body of a septic substance derived from an abnormal secretion. I suspected the prime source of the "lymph," as it was called, from the first; in which discernment there was no great merit, since I had witnessed the action of the same, or a similar, substance, to which I gave the name of *Septine*, in the course of experimental work conducted over a quarter of a century ago,* and in a short note

* *Septine.*—In these researches I extracted, from various diseased secretions, the substance to which I gave the name of Septine. I first obtained this substance in the form of a crude extract, and afterwards

to the *Lancet* I communicated that the poison, without doubt, had its origin from a tuberculous source. But whether bacillus secretes the poison, or the diseased body secretes it and bacillus is the scavenger of it, is not the question at this moment; the question is, the action of the substance and its service. In discussing this vital question, let it be conceded, for argument's sake, what for strict truth's sake ought not to be hastily conceded, that some effect leading to cure in certain stages of pulmonary consumption might result; let it be admitted as possible that the favourable views which the friends of the "cure" see are not entirely illusory; nay, go one step further, and let a bacteriological miracle

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in a much more concentrate, and, if such a word is applicable to such a substance, "pure" form, from peritoneal fluid removed from a patient, under the care of Sir Spencer Wells, suffering from what we then called "surgical fever" following upon the operation of ovariotomy. I also made it into an available form with glycerine. At the meeting of the British Medical Association at Leamington, on Thursday, August 3rd, 1865, in a paper on the hypothesis of the origination of disease by zymosis, I related the details of experiments in which I had isolated the same as "pyæmic poison," specimens of which were exhibited. This poison, I explained, could produce two kinds or classes of symptoms: one in which symptoms allied to typhus were presented; the other in which acute inflammatory fever, with subsequent fibro-purulent deposits, was declared. The argument was, that, although difference of symptoms followed the action of inoculation or injection of septine,—for it had, like viper venom, no effect when taken by the mouth,—there was, probably, only one poison primarily, and that the modification of effect depended on atmospherical variations acting upon the septic matter. In this paper I also pointed out, for the first time, that we ought to distinguish between the primary symptoms caused by a primary septic substance and the after-symptoms caused by secondary products, like lactic acid, formed under a modified zymosis, as the result of febrile disturbance.

As to the primary poison itself, I maintained that the force of its production was not of foreign cellular origin exerted by growth of new cells, but the force of secretion; and in regard to its nature I looked upon

be admitted. Let the Hahnemannic theory of *Similia similibus curantur* be admitted; let the infinitesimal dose be admitted; and—most curious admission of all—let the queer theory of that queer old genius Hahnemann about the typical development of disease as Psora be admitted; in plain, let the whole "rank homœopathy" of the process, as it would have been stigmatized a few years ago, be admitted; and then the *cui bono?* Is this art of inventing a new disease or series of diseases, in order to cure or prevent those diseases which seem to occur spontaneously, a true and useful art in its application to medicine? It may be science; for science of the highest may be of the lowest

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it as an organic alkaloid of the ammonia type,—a view derived from the observation that the substance would pass through the dialyser, and could be made to form salts with acids as morphine could. Further, I showed that the substance was neutralized in its action on healthy animals by bromide, iodine, and mercurial salts. In the discussion which followed my paper at Chester the late Dr. William Budd challenged the argument in this way. He said he held in his hand a phial containing the virus of smallpox. With that virus he could propagate the disease, smallpox, through hundreds of thousands of generations. This was because that virus was "contagium vivum," and he denied that I, with my charge of chemical substance, could do anything of the kind. The result of that challenge was, that I not only tried, but did propagate disease from one animal to another with this very organic substance, and showed that by propagation the intensity of the virus increased instead of decreased. I afterwards obtained a virus from the urine of a patient suffering from tetanus, which virus produced tetanic symptoms in the lower animals just as strychnine does. In a third set of inquiries I obtained from the expectoration of phthisical patients a similar virus to that obtained from the pyæmic case, in so far as the effects were concerned, after its subcutaneous injection into the bodies of inferior animals.

To this virus then I gave the hypothetical name of *Septine.* I looked upon it, and I still look upon it, as a specific virus, common to all the infectious diseases, and as a secretion produced under the influence of the nervous system as other secretions are. Hence I have ventured

and most mischievous kind, as applied to arts affecting the welfare of men, and to medical art above all. Suppose, then, this new development to be of science, say the highest science, is it for the good of mankind in general and of sick mankind in particular? Homœopathically, the application of the science to medical art may be in accordance with homœopathic principles. But homœopaths do not seem to see it; and I believe, honestly, that they are wise in their generation. They do not want through science to bring into the world *paratoloiditis* or any new disease of that sort, even though that renowned malady should, under certain limited circumstances, kill its grandmother phthisis pulmonalis. Neither do they wish, in discredit of their theory, to see diphtheria, scarlatina, smallpox, typhus, and the rest

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to speak more than once on the nervous origin of the infectious diseases, tracing them to derangement of glandular function, comparing the virulent product to the secretion of a venomous insect or reptile, and arguing that, under bad sanitation, such poisonous developments may take place, not spontaneously as some express, but, simply, as the result of bad sanitation, or of nervous injury or shock. In appending this note I wish specially to state that the last thing I should wish would be to lay any claim to priority in the use of the fluid called "paratoloid" as a curative principle; for, as will be seen in the text, I do not accept as good practice such a method of trying to cure. I wish simply to state the facts of an old endeavour to trace out the nature of the virus of the communicable diseases, which the effects of the late experiments have brought back to my mind.

Readers who may be interested in the theory above noticed of the glandular origin of the contagious diseases are referred to the report of the meeting of the British Medical Association at Leamington, in the *Journal* of August 12th, 1865, pp. 165, 166; to a lecture on "The Poisons of the Spreading Diseases" (London: John Churchill & Sons, 1867); to a Presidential Address to the Sanitary Institute, published in *Nature*, 1877; and to the article on "Zymosis, or Ferment as a Cause of Disease," in my work entitled "The Field of Disease" (Macmillan: 1883), Book III., chap. viii., pp. 815-62.

parading under new names, and killing their prototypes in order to establish new types. They are wise men. I cannot accept their dogmas now any more than ever I did, but I am bound to acknowledge their discretion *re* paratoloid disease and all its congeners. Better, far better, surely, to see the lady Hygeia continuing to scotch the snake, than to see the snake scotching her, getting out of her bonds, and going about at large scotching everybody with new infections.

To common sense the whole proceeding of meeting one fatal disease by the implantation of another is the *argumentum ad infinitum* in the treatment of disease. Are physicians to become conjurers, trained by the quickness of hand to deceive the eye? If they are, let them declare their new vocation, and give up the old art of healing to babes and sucklings in that art. Physicians, as true scholars and men, live for two purposes, and two alone. They live first and foremost to trace out the causes of disease, and to remove those causes; not by fiddling in minor keys of experiment, but by coming out like men into the study of the wayward habits of ankind in the highways and byways of humanity; by teaching, far and wide, that such fatal diseases as pulmonary consumption, and all the contagious groups of diseases, are born, not of bacilli, but of ignorance; and that to sustain, artificially, a Pestis Bacillorum, if they could do such a thing, would be but to sustain pestilence in new and varied types, and to graft on their species communicable diseases of a new character.

I re-assert, for my part, that inoculation is bad sanitation, nothing more and nothing less; and I am sure that if for a time it showed some specious appearance of success, as a prophylactic, it would soon become impossible as a practice. Think of the proposition that has been advanced

as a forecast on this paratoloid business, that in good time medical men will be able to protect humanity, from all the contagious diseases, by inoculating it, during its days of infancy and childhood, with different specific poisons, "pure" cultivations from diseased products. Think of the awful misery of such an accursed childhood; think of the popular indignation whenever death followed such inoculations; think of the Acts of Parliament that would have to be contended for, in order to carry out such a course to its legitimate end; think of the hatred that would spring up against the professors of medicine on every side; think, in this country, of the anti-vaccination opposition some sixteen times multiplied;—think of all these dangers and difficulties, and compare the practice that would foster them with the glories of that truly noble art which leads to health by healthy manners, by purity of action, and by simplicity and beauty of life in the living bath of cleanliness and goodliness.

These last-named glories are the primary duties and teachings of the true physician; and with these he ought to feel fully content. If he could do more he would do worse. If he could say, with certainty, to a man or woman, "Submit to an operation I can perform on you, and I can assure you of immunity, for a stage of your life at least, from some particular disease," he would only make that person depend on an artificial for a natural means of protection, and would merely engender in him a neglect which, in the end, would increase disease and heighten mortality. Mankind is careless enough as it is, ready enough to repose for security on charms and mysteries, without adding to mystery, and without trying to restore the medicine-man of the savage to high place in the ranks of the civilized. Towards the highest duties of preventive art we have of late years toiled with

labouring feet and with marked success. Let us not be led back one inch by any charmer, charm he never so shrewdly. We have lived to recognise the truth that a human being may go through the whole of his life without suffering from one of those plagues which were once believed to be the inevitable curse, the dispensation of a merciless Providence, which struck open the door of the palace of the prince and of the hovel of the peasant with equal foot; and, having seen that such fate can be averted by natural means, it is our office to learn to extend a true, though yet exceptional, safety up to the grand old prophecy, when "there shall no more be an infant of days, nor an old man who hath not filled his days."

The second duty of the physician is to meet disease when it has stricken mankind; to know disease when it appears, and to cure it. Aye! cure. Cure, not by check-mating one disease through the implantation of another; not by raising up a Frankenstein for every affection that threatens life; not by introducing into the living temple treacherous organic poisons, determined to live on what they feed on, and unrestrainable when once they have started into birth and full activity; not by trying to gamble with disease and make up a book that must for the occasion, at all hazards, win; but by learning how to place the sick body under the conditions most favourable to the continuance of life; or, highest development of skill, *under conditions in which it cannot die.* This is cure of disease by an art second only in divinity to absolute prevention of disease; an art to be attained if we truly try after it; not by the vulgar and illogical process of implanting one disease on another, nor by inventing new diseases and new conditions of disease; but by learning to neutralize current diseased conditions at

the moment, and so placing the body that it shall swing round from erring to natural movements of life.

In plain words, curative medicine as well as preventive must be worked out altogether on a natural system, the governance of which, in the hand of every practitioner of medicine, may be as correctly served as is the governance of the steam engine by the hand of the driver, but with a more elaborate knowledge proportioned and proportionate with the more intricate and more elaborate nature of the vital endowments of the mechanism of the animal body. Then curative and preventive medicine will run together to form one omnipotent science and art of healing; then there will be neither humoral pathology nor solidism; but one system, physiological, pathological; disease unity with variety of phenomena, resting on very few aberrations; and prevention and cure a perfected knowledge of cause and effect, with control of both. Twenty years ago we were steering well and steadily towards great principles on the preventive as well as on the curative side of medicine; then there crept in the wild enthusiasm for bacteriological research; research good enough in its way, as a piece of natural history, and as disclosing some curious vital phenomena developed under morbid states of the organic structures and the blood; but a positive insanity when accepted as the one absorbing pursuit, restoring the humoral pathology, ignoring nervous function, leading to Babel with its utter confusion of tongues, and separating, for a time, our modern art of cure from the accumulated treasures of knowledge, wisdom, and light of over two thousand years.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."

"DIPHTHERIA IN SALFORD."

[BY CHARLES E. PAGET.]

MR. PAGET, as Medical Officer of Health for Salford, has published a special report on the recent prevalence of diphtheria in the district under his able supervision. It is a most laborious and admirable document, the charts, maps, and diagrams the best probably that have ever been brought together on this subject. Unfortunately, the results, truthful as they are, are mostly negative as to cause of diphtheria. Thirty years since there was a considerable amount of diphtheria in Salford, and subsequently a certain diminished but recognised prevalence of the disease until four years ago, when it began steadily to increase, and from being limited, passed into the epidemic form. The origin of the outbreak cannot be exactly determined, but there is good ground for believing that it commenced in the last quarter of the year 1887, and spread from the circumstance that a large number of the cases of the disease were of so mild a type as to escape recognition of their

true nature; they were, therefore, allowed to pass on to convalescence without precautionary measures being taken to prevent them serving to spread the disease. Mr. Paget excludes milk supply as a cause; also infection from domestic animals, and even soil pollution as a direct causative connection. In short, although the spread of the disease must have been assisted by the bad conditions of house drains and of sewers, the main cause was the personal communication, in which the schools played a leading part.

"THE STRUCTURE OF THE CENTRAL NERVOUS SYSTEM."

[BY LUDWIG EDINGER, M.D.]

THIS volume (F. A. Davis) contains twelve lectures, translated by Willis Hall Vittum, M.D., and edited by C. Eugene Riggs, M.A., M.D. The lectures, excellently translated and edited, are those of a teacher who has made the central nervous system a subject of closest investigation; and the introductory chapter on the history and methods of investigation is of itself remarkable, although it is compressed into twelve pages. On first opening the book it seemed to me as intended for teachers only; but as I got into it I began to feel that in its bearings on practice it is as useful and interesting as it is in a purely physiological sense. The descriptions of the parts which enter into the construction of the "medulla oblongata," and the final review on the "pons," are singularly instructive. The work throughout is well illustrated, and will for many years remain a standard work of its kind.

"COMMUNICABILITY OF CONSUMPTION."

[BY G. A. HERON, M.D.]

THIS book (Longmans) is an out-and-out book; a book without the slightest concealment of prejudice, and without revealing the remotest fear of the results that would spring from the practical application of the conclusions of its author. For these reasons, if for none other, it should be obtained, read, and studied by all who are concerned, and who are not concerned, in the question of the "evidences of the communicability of consumption." There are, however, other values in this treatise; it is well written, and, admitting its premises, it is logically constructed. To Dr. Heron the disease, consumption, is just as certainly due to the presence of the tubercle bacillus as scabies is due to the acarus scabei. The disease, consequently, is, he holds, communicable by contact, and all our views about the hereditary nature of consumption must yield to this assumption. "Tubercle," he says, "in all its forms is an inoculable disease;" but "without the presence of the tubercle bacillus or its spores inoculation would be impossible;" and "since that organism became known to us no one has succeeded in showing any instance where tubercular disease has been caused without the agency of this bacterium." It is always refreshing to find an author stating a proposition in this absolute form. It is like Euclid at the outset, but between that and the Q.E.D.! There's the gap! Nevertheless, those who are not convinced may profit from the argument if they will, for there is much even for his opponents to learn in Dr. Heron's outspoken pages.

"HEREDITY, HEALTH, AND PERSONAL BEAUTY."

[BY JOHN V. SHOEMAKER, M.D.]

DR. SHOEMAKER is making for himself a good name in the old country, and assuredly his rich volume on "Health and Personal Beauty" will add largely to his reputation. There are thirty-seven chapters in the book, some of them with the most attractive titles, such as "Grace the Crown of Beauty" and "The Skin as a Beautiful Tissue." The chapter, however, that has gratified me the most is that on "The Effect of Environment and Training on the Physique." In this chapter we are told that, after the great Civil War in America, the Government made a compilation of remarkable surgical and other matters, from some of which it is to be inferred that change in the human organism may be so rapid as to be conspicuous even within the space of an ordinary lifetime, and that it often lies within our power to control its duration and amount. In the chapter on "Grace and Beauty" movement is put before form; "it is the movement that makes grace." In another chapter on the "Art of Walking" the æsthetic side of the question is ably and almost playfully dealt with. "The gait and port" of the Spanish woman is the ideal of the author of "Health and Personal Beauty," who holds that mere human butterflies, flitting from flower to flower, with no more exercise than is sufficient to enable them to sip, in quickest succession, the sweets of life, are the necessary models of beauty. The butterflies may want ballast, but that is not the question; they have fresh air, exercise, best food, and constant change,—influences which directly promote beauty; and, in addition, they have "the greatest cosmetic of the world," absence of care.

OPERÆ PRETIUM EST.

AMONGST the hundred books which have come in during the past quarter, and to which I have time now to refer as amongst the best, the first is Sir James Paget's "STUDIES OF OLD CASE BOOKS" (Longmans), a most taking volume of experience as well as learning, of wisdom as well as knowledge. Sir James prefaces a kind of apology for case book literature; yet surely it had been a pity, amounting to a sin, if this record of his case books, all too brief, had not been left to the world. The little chapter on "The Use of the Will for Health" is of itself an hour's wealth.

"APHORISMS IN APPLIED ANATOMY," by Dr. Thomas Cooke, is a small and handy volume of anatomy for final examination, with "OPERATIVE SURGERY, including Typical *Viva Voce* Questions in Surface Marking," etc. (Longmans). The work is that of one of our ablest anatomical teachers; one who, under a free system of medical teaching, would stand as an anatomical professor of the highest rank, in which position he would only be in public esteem generally what he is really amongst all who know him and his devotion to his favourite art. It is unnecessary to commend this little volume, since it is a necessity to all who, climbing the first steps of the professional ladder, are trembling as the examination table comes near to view.

A very useful handbook of illustrated lectures on "NURSING AND HYGIENE," by Dr. Lawton Roberts (H. K. Lewis), commends itself as a companion to a volume by the same author on ambulance work. The instruction to the sick nurse is practical, convincing, and simple.

Turning to works of a different character, I cannot refrain noticing with much pleasure Mr. Francis Vacher's "STORY TOLD TO A LITTLE CHILD" (Simpkin, Marshall & Co.). It is sweetly pretty, and perfect as a work of art of its kind. A small volume of "NEW AND ORIGINAL POEMS," by Dr. Edward Allingham (Reeves and Turner), also calls for attention. The poems vary considerably in value, but there is in some of them good ring.

TO CORRESPONDENTS.

SPECIAL NOTICE.—*Many readers of the* ASCLEPIAD *having requested permission to have, for framing or for the album, extra copies of the autotype portraits published with the biographies, the Author has pleasure in stating that every one who sends him intimation, to* 25, *Manchester Square, of having complied with the terms of publication as a subscriber, will receive at the close of this year separate copies of all autotypes published in the volume for* 1891.

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The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

The Appendix of Acknowledgments, including Books for Review, and Exchanges, will be published in next Number.

February 15th, 1891.

ON PEROXIDE OF HYDROGEN: A PHYSICAL-MEDICAL RESEARCH.

CONTINUED FROM A RESEARCH COMMENCED IN THE YEAR 1856.

PART II.—PHYSIOLOGICAL AND THERAPEUTICAL.

AT the close of the last chapter (Asclepiad, No. 29, p. 13), the action of purulent matter on the solution of peroxide of hydrogen was described. It was shown that the presence of purulent matter leads to a rapid evolution of the oxygen, and that during evolution the pus cell is destroyed. To the facts there stated, some further facts of practical importance, therapeutically, should be added.

The Peroxide and Blood.

Before I enter into any new details, I would submit a brief exposition of an analogy which has occurred to me between the oxidation of water manifested in the peroxide and the oxidation of blood. It seemed to me, from the first, that the additional oxygen in the peroxide is held in the same kind of loose combination with the water, as the oxygen of the blood is held in the blood stream. The water of the blood does not give up its own oxygen, but it conveys to the tissues readily enough that which lies

outside it in the corpuscles, as they come into contact with the tissues, just as the supercharged water of the peroxide solution gives up its oxygen, when it is brought into contact with certain catalytic substances. Under this impression I conceived an idea of establishing a synthesis of blood, which idea I partially carried out, and shall, in a supplementary portion of this essay, describe, together with some additional later work in the same direction, not at this moment completed. Meantime, it is necessary to refer to the view here noticed, since it enters into part of the argument which follows, on the influence of purulent material on the peroxide, and of the peroxide on it, to which subject I now return.

The Peroxide and Purulent Matter.

The influence of pus on the peroxide is modified by temperature. The disintegrating process is quickened greatly by heat. At fever heat of 105° Fahr., the escape of the gas, even when the solution is one of five volumes only, is so intensified as to cause a brisk effervescence. While the escape is in progress, the purulent fluid lies at the upper surface of the solution, as a rather thick and yeast-like covering. After a time, the covering contracts with or from escape of contained gas, and the mass assumes a cheesy or caseous appearance, resembling the caseous substance which is found not infrequently in a chronic abscess. Later on the mass sinks to the bottom of the containing vessel, and if left there, with a little fresh peroxide added to it, passes almost altogether into what we should call, in disease, a sanious fluid. In short, the changes, throughout, are singularly like what we observe in a deep-seated abscess of long standing, and indicate that the mode in which pus is changed, in chronic abscess, is a slow oxidation, the oxygen being derived from the

blood. The lesson is pregnant of suggestion. Whenever pus is present in large quantities in the body, the deoxidation of blood that must be going on fully accounts for the depression that is so often fatal. In confluent smallpox, when the wide cutaneous surface is covered with pustular eruption, when the skin is one sheet of purulent matter, it is no wonder that the blood is deoxidized, and that there should be those signs of asphyxia which the old physicians called black or suffocative variola.

I take it as all but certain that what is designated hectic fever is brought about in a similar manner. In conditions during which hectic is a periodical phenomenon, purulent material is formed in the tissues, and, accumulating to a given point of saturation for becoming active, begins at last to liberate too freely the oxygen from the blood. Then occurs the febrile stage, a stage which continues until the purulent material is reduced to a neutral condition by oxidation. After that the febrile heat ceases; a copious supply of water, formed, as a necessity, during the period of excessive action, is liberated by the skin, and the final result is extreme exhaustion, which continues until, in the process of reaction, there is a new formation of purulent substance, reaccumulation, and again destruction. In all febrile states leading up to purulent formations this, probably, is the natural course towards recovery or death.

After observing these active effects from purulent material, I set myself to learn what would be the action of other substances, which sometimes play an important part in the phenomena of disease; and, with this object in view, I fitted up a small chamber like that used for the incubation of eggs, in which I could secure and sustain a temperature varying from 96° Fahr. to 110°, at any degree within this range, for any requisite period of time.

Into this chamber I introduced flasks charged with the peroxide solution combined with the substances to be subjected to experiment, exposed them to a fixed temperature, and, collecting the gases evolved and measuring them, determined the degree of action in each case. I followed also another line of enquiry. After charging a flask with the peroxide solution and with an organic substance that would not produce separation of oxygen, I closed the flask firmly with its contents in combination with a minute division of another substance, like black oxide of platinum, which would, by its presence, cause liberation. By this means I was able to trace the course of oxidation, under pressure, of different organic substances, as well as the resolution of those substances into new combinations.

Infected Flasks.

In the course of these last-named experiments, a singular observation was made, which, even if it lead to nothing of moment immediately, even if it be but one of those mites in science which Benjamin Rush speaks of, is worth recording. I noticed that sometimes the flask in which the experiment was made would afterwards evolve oxygen from the peroxide solution, although it had been emptied of its contents, washed, and rinsed over and over again with fresh water. I called these flasks infected flasks, and I showed one of them at the meeting of the Medical Society of London, before which this paper was read. I had a flask which retained this property for years, and would permit of having distilled water boiled in it without losing the property. The explanation of the phenomenon is, I think, simple enough. I suspect that the glass possesses, in these instances, a certain degree of porosity, and that the disturbing body is left in these pores in minute subdivision of an infinite fineness. But simple as such an explanation

may be, it carries with it a lesson. It shows that minute disturbing causes may be obscurely laid by, and, under favouring conditions, show an activity as strange as those mysterious phenomena called vital and hereditary.

The Peroxide and Decomposing Matter.

After observing the action of the peroxide with purulent matter, I extended the enquiry. I found that some decomposing substances, such as fibrine, which ordinarily release the oxygen from the solution, release it more rapidly when they are undergoing decomposition than when they are in the fresh state. Fibrine itself, when it was fresh and washed to perfect whiteness, would slowly liberate the oxygen, but if it were put into the solution when it had commenced to decompose, the action was greatly expedited. Another fact also was elicited, namely, that with the more rapid action the disturbing substance itself—the fibrine—underwent more rapid dissolution, and at the same time lost the offensive odour due to decomposition. If the fibrine were put into the solution in the fresh state, it would remain for weeks unchanged in character at a moderate atmospheric temperature; but if it were put in during process of decomposition it was broken up, and became sanious, like pus under the same influence.

I wondered if bodies which do not set the oxygen free when they are immersed in the solution in their fresh state would do so if they were immersed in it in the state of decomposition. Albumen was tested. Albumen, obtained from the serum of blood, which, in its fresh state, had no effect, was allowed to decompose in water and was again tested. This time there could be no doubt that liberation, with quick dissolution and removal of putrefactive odour, took place, but why did not appear

obvious. It seemed to me as if something had been unloosed by the decomposition which created the effect; and seeing the action of carbonic acid in the same direction, I suspected that it was the disturbing cause, but I do not find in my notes any proof from experiment. I suppose, therefore, it was a suggestion left for elucidation.

Some other experiments bearing on albumen are of moment. I discovered that if fresh albumen were mixed with the neutral peroxide solution, and then with organic substances that would liberate the oxygen, pus for example, the albumen would quickly resolve and pass into solution. These substances, in short, act like ferments in the presence of the oxygen: they set the active oxygen at liberty, and then it begins to attack and break up the compounds with which it comes in contact. For the same reason that it decomposes potassium iodide in the ozone test-paper, it breaks up the products of decomposition, in which respect it acts like ozone, and I shall show how these observations led to the employment of the solution, by external application and in form of spray, in the treatment of fœtid surfaces, and in the purification of the air of sick-rooms. These results of breaking up of tissue carried me further on in similar lines. I enquired as to the action of the peroxide on decomposing saccharine matter. Solutions were made of grape sugar, and the products evolved by fermentation by means of yeast were taken as a standard. Next, the changes in fermentation in the presence of peroxide were noted; and in continuation of the same course of experiment, the yeast was substituted by such bodies as black oxide of platinum, oxide of manganese, permanganate of potassa, muscular flesh, portions of the structure of the lung, cellular tissue, and fibrine in the moist state. If the peroxide were left to act on the grape sugar alone,

at 100° Fahr., carbonic acid was evolved and the sugar was resolved into its secondary compounds; but the process was slow. Something was required to give the start to the action in order to make it lively, and then it went on without delay. It was astonishing to see what a very little would effect the start, and how long the activity would continue, when the start had commenced, without further provocation from what we may call the fermenting or provoking body.

A strange practical problem lies here, connected with what may be called starts in new directions, during chemical action, and of progressions in the new directions in continuance of the initiative. To explain best what I mean on this curious point, I will put an elementary fact into the foreground in illustration. If the solution of peroxide of hydrogen be simply exposed to heat in a clean glass vessel, it can be heated up to boiling point before the additional oxygen will begin to go off. At boiling point the oxygen will be liberated, and, once started, it will go on being liberated until all of it has escaped, although it be rapidly cooled down to the common atmospheric temperature. The start has given the direction, and the effect continues to the end, until every part of additional oxygen has made its escape, and nothing but water or monoxide of hydrogen remains. In another way the same thing takes place under the catalytic action of substances added to the solution. A point of black oxide of platinum will start the action by its presence, and the action will continue. Again, the presence of some decomposable organic substance will start the action, and will keep it up so long as it is itself decomposable. Lastly, and this, physiologically and pathologically, is most important of all, the process of disintegration being set up in matter of an organic

kind, which in its natural condition would be neutral, secondary products will sustain the action so long as any oxygen and any product remain to react on each other.

I cannot press this conclusion home too far, in its bearing on the study of disease. If the blood in the living body holds its oxygen, as I have previously suggested, in the same manner as the water of the peroxide solution holds its oxygen, then we can see why from one small point of infection general infection may follow in the body. The locally infected spot becomes the seat of a change by which the formation of a new and disturbing product is formed; that disturbing product carried into the blood increases naturally, sets free more oxygen, enlarges the process of action, and at last brings the whole of the vital tissues into the catastrophe of fever heat, forced oxidation, with all the consequences of dissolution, and with the ordinary aliments of the body feeding the unnatural fire, until, from failure of oxygen, or from failure of fuel, the overaction ceases.

Organic bodies of a fixed nature, like albumen and sugar, are thus rendered decomposable in the presence of oxygen in continuous line, so soon as their decomposition has once been commenced. It is, I believe, just the same event as that which takes place when we set fire to a piece of wood. The fire may be started from the minutest point, but feed that point with oxygen, and the fire goes on until all the fuel is decomposed. The organic substances, when once the change is started in them, burn away like wood, not truly with the fury of heat exalted into flame, for the presence of undecomposed water prevents that, but with radiation of heat—fever—as a part of the process. Fever, in brief, is active combustion in water.

These facts bring near to us, and into one common view, the processes both of catalysis and of fermentation.

When, on mixing peroxide solution with decomposable matter, we start oxidation of that matter, from the addition of a point of black oxide of platinum, we set up movement by what we must designate catalysis. We cannot fairly call this fermentation, because the exciting substance is inorganic. When, however, we start the change from a point of purulent matter, we must designate the process one of fermentation. We cannot call this catalysis, because the exciting substance is organic, and is itself capable of destruction in the course of the disturbance it has set up. Yet the two processes are, practically, the same.

Another series of experiments were made with dried undecomposed pulverized fibrine and dried undecomposed pulverized albumen, both obtained from the blood of an ox, and both reduced to an impalpable powder. The fibrine added to the peroxide liberated the oxygen, the albumen exerted no action whatever. By the fibrine I could excite direct oxidation of any organic substance of a decomposable kind with which the solution was commingled; with the albumen I could excite no action whatever, until the process was started by some organic or inorganic disturbing body. Here we are brought into the actual presence of catalysis and of zymosis, in organic bodies, as the same processes; and, perhaps, one day there may be found here the ground of reconciliation between the representatives of the popular school of microbians, and the broader, but for the moment unpopular, school of physicians.

THERAPEUTICAL OBSERVATIONS.

From these debatable subjects I turn now, for a time, to others more directly practical, confining myself, in the present number, to the earliest observations on the peroxide of hydrogen as a therapeutical agent.

Ozonic Ether.

In turning to therapeutical action, let me notice in the first place a few new facts bearing on modes of administration of the peroxide. Some time about 1863, I made an observation on the behaviour of the peroxide with ether. In agitating a thirty-volume solution of it with anhydrous ether I found that the ether became ozonized, and that it was in this way possible to make a new and useful medicinal preparation. To the preparation so formed I gave the name of ozonic ether, and from the time when it was first announced until the present time it has been utilized. The late Dr. Day of Geelong, in Australia, sent to me early for its mode of preparation, and having learned to use the ozonic ether in his own practice, devoted to it much study up to the end of his valuable life. He warmly supported my application of the fluid in the treatment of whooping-cough, as well as its use for inunction after scarlet fever. It was he who first employed the compound with guaiacum as a blood test.

Peroxide Spray.

The admixture of the peroxide with a volatile compound, like ether, led me to another modification, namely, the administration, by inhalation, of the oxygen evolved either as oxygen, ethereal oxygen, or ozone, for I soon learned that it could be administered in all these states. This was a distinct advantage, as it enabled me to move from the mode of administration by the mouth to a larger, and, in cases of lung disease, a more direct method. Taking further advantage of the ozonic properties of the peroxide solution, I introduced it in 1865 at the meeting of the British Medical Association at Chester for the deodorisation and disinfection of rooms in which patients suffering

from infectious diseases are under treatment, and also for application as spray to fetid, ulcerated, or decomposed surfaces, as in diphtheria, ulceration of the throat, and gangrene.

Employment in Treatment.

In the paper of 1862 I reported many cases of disease, in which I had applied the peroxide as a remedy; and in respect to all the forms of diseases then specified I may say that in the long experience which has succeeded I have nothing of moment to retract, little to modify, much to emphasise and support.

In Diabetes.

I have many times prescribed the peroxide solution in diabetes since 1862. In four cases of the disease, in members of our own profession, the patients were all satisfied of benefit from it, one of them taking it for a period of eleven years. The results have always been that it reduced the quantity of sugar, but did not reduce the quantity of water eliminated, unless it were combined with codeine, with the use of which Dr. Pavy has made us so familiar. Codeine *three* grains, alcohol (sp. gr. 0·830) *two* fluid drachms, solution of peroxide of hydrogen (10 volume strength) *two* fluid ounces, distilled water to make *twelve* ounces. This forms a mixture of which half a fluid ounce may be taken three times a day in a wineglassful of water. It is the most useful medicine I have ever prescribed in diabetes.

In Phthisis Pulmonalis.

In the treatment of *phthisis pulmonalis* the solution has been constantly in use, and from it I have found the greatest service. In some cases in which the solution has been borne in large doses—ʒiv of ten-

volume strength well diluted with water—during the early stages, it has led to such good results, I might almost have been pardoned if in younger and more enthusiastic days I had declared in it the discovery of a specific. Happily, a natural fear of the *experientia fallax* held me back from such a course; but the fact ought not to prevent me from stating that in the first stages of phthisis it is of all remedies I know of the most valuable; and, that in the later stages, when dyspnœa is the distressing symptom, it is, especially in the form of ozonic ether, the remedy that gives most relief. It soothes, and, as if it supplied oxygen to the blood, seems to act vicariously for the purpose of respiration.

In Pertussis.

I repeat all I have previously said about the value of the peroxide in the treatment of whooping cough. I am sure there is nothing so near to a specific for this affection. The only difference I have instituted in the employment of it in this disease consists in prescribing it in the form of ozonic ether. I prescribe it now for children in this form, in doses of from ten to sixty minims, diluted with half a fluid drachm of spiritus tenuoris and four drachms of water, to be taken four times a day, with a little more water if that be preferred. But I also administer the ozonic ether in this disease by inhalation from the Siegel spray inhaler. In one instance, where the paroxysms were as severe as I have ever seen them, they were arrested at once by this inhalation, and were held in such control by continued inhalations of it that what may be called a "cure" was the immediate result.

In Asthma and Angina Pectoris.

It will be remembered that the power of the oxygen to control muscular action has been referred to, and,

once more, I repeat with still more earnest insistence this fact. In practice I have availed myself of the peroxide as ozonic ether, not only in whooping cough, but also in asthma and in angina pectoris. Here the ethereal solution can be used in its simple form, or combined with amyl nitrite, or other volatile body of the antispasmodic class that will admix and diffuse with ether. In these affections I prefer to administer it by inhalation, but it can also be prescribed by the mouth in diluted spirit solution.

In Syphilis.

I repeat the value of the peroxide in secondary syphilis, and in all the chronic stages and conditions of syphilis in which mercury or potassium iodide would, usually, be resorted to. It seems to me, in these cases, to have an intermediate action between mercury and potassium iodide. It is also useful in indolent syphilitic eruptions, and it has the great advantage over both mercury and the iodide, that it leaves no after-effects of an injurious character. Salivation from large and frequently repeated doses is soon recovered from after it is withdrawn, as if the ptyalism had been induced by a volatile substance. It aids the action of mercury, rendering much smaller doses of that remedy effective. The property exemplified in this action is that of elimination by excitement of glandular function; an excitement probably always sustained by the oxygen of the tissues in normal states of health, and only accelerated by the active oxygen. I have of late years become so confident of the value of the peroxide in the treatment of syphilis under the conditions in which mercury or iodide come ordinarily into use that I have employed it as a matter of course. I use it in the primary as well as the secondary form of

the disease, and for those cachectic lesions in the syphilitic constitution to which the term "tertiary" is applied there is, I think, no remedy equal to it. In obstinate cases of ulcer and cutaneous eruption it goes admirably with the mercurial bath, and may be prescribed in doses of from two to four drachms three times a day with full doses of fresh cinchona infusion. The solution also has its local use. It makes an excellent injection into the nasal cavity when there is syphilitic ulceration of the cartilage and spongy bones, syphilitic ozœna.

Local Use for Sphacelus and Purulent Exudation.

In the paper of 1862 I recommended the use of the peroxide for external application in cases of surface decomposition and ulceration. This followed, naturally enough, from what I had observed in regard to the action of the peroxide with decomposing and purulent matter. I have to emphasise this use. I will name one case alone in illustration. I was called to see a gentleman advanced in life who was suffering from moist senile gangrene of the foot. The odour from the dead extremity was most offensive, and the separation slow, although there was a fair line of separation. I had the foot wrapped in cotton wool, holding the peroxide solution slightly acidulated with dilute hydrochloric acid. The offensive odour was at once removed, and the process of separation went on rapidly. The foot soon fell off, leaving an excellent healing surface. The patient lived many years afterwards in fair health, and died at last of mere old age. In examples of bed sore the same useful service is rendered.

In Gonorrhœa.

The local action of the peroxide in acute purulent affections is quite remarkable. In such a specific

purulent affection as gonorrhœa I have seen an injection of it transform the purulent into an aqueous discharge in forty-eight hours. In this disease the solution for injection must be of neutral reaction and weak; in the beginning two fluid drachms of a solution of three volume strength is amply sufficient, and that may be reduced in strength by further dilution with water, if it causes pain, or if effervescence from the escape of oxygen is too active.

In first employing the solution for injection in gonorrhœa, I prescribed a solution of five volumes, which was rapidly effective, but rather painful. I then reduced the strength to three volumes, and found as good an effect without pain, and with very little loss of time. In gonorrhœa it is also good to have the solution applied to the glans as a wash or lotion for the destruction of purulent matter that may gather on the surface and under the prepuce.

USE IN PLEURO-PNEUMONIA.

In observing the special action of the peroxide of hydrogen in the above and other diseases affecting man, it has occurred to me that it might exert the same useful specific effect on animals (cattle) suffering from pleuro-pneumonia. In this disease the solution could be injected by needle injection into the pleural cavity of the infected animal; but a second puncture would, perhaps, have to be made to give exit to the gas liberated, should too much gaseous pressure be inflicted on the lung. From absorption of oxygen by the lung there could be no danger beyond emphysema, and that would not occur if care were taken to avoid pressure. A ten-volume neutral solution would be of sufficient strength.

*NATIONAL MAIN DRAINAGE.**

SINCE the earliest days in which I have taken part in the work of sanitation until now, there has been no subject in the whole range of sanitation on which I have been consulted so often and so earnestly as that of drainage. Sometimes the authorities of a city or town have communicated with me on the subject; sometimes, the people of a village; sometimes, the builder of a district or suburb of a place; sometimes, the owner or builder of a house. Occasionally a legal dispute has led one or other of the litigants to me. A house or a block of houses has been drained by the proprietor, and the tenant or tenants finding, after a time, a defect, and taking proceedings, my services have been called for to inspect and report. More than once I have taken part in a sanitary inquiry, of a purely scientific kind, bearing on principles of drainage, and on investigations into the efficacy of certain methods that have been adopted for drainage. I could fill a good-sized volume of reports of experiences of this nature, both at home and abroad; the outcome of it all, however, would be that no results have been so obvious as two results—

* A Presidential Address to the Association of Sanitary Inspectors, delivered at Carpenters' Hall, on Saturday, May 2nd, 1891.

contradiction and utter chaos. I do not conceal for a moment that in the course of these experiences I have, over and over again, been obliged to submit to the sanction of practices opposed to my knowledge and conviction of what was scientifically right. It is the truth that, at this very moment, in the latest scheme for drainage in which I have been professionally concerned, in consultation on the spot with one of our best and best-known practical architects, the plan of drainage to which we have had to submit is the plan to us most objectionable, namely, drainage into a cesspool, with overflow into a little watercourse or brook. Why, some one will say, why all this compromise? I answer. Unnecessary necessity; that necessity which comes from bad education on the question in hand; general ignorance of sound principles under which every one might be benefited; and, that selfish want of common sense which stands so often in the way of useful measures for the common good.

Drainage as a Matter of Expediency.

Everything in drainage goes by and for expediency. There is a watercourse near, therefore drain into it; the sea is at hand, therefore, of course, drain into it; there is neither a watercourse nor sea near, therefore dig a cesspool or make a sewage farm; but the sewage farm will taint the air or will give up water that will taint an adjacent stream, therefore let the cesspool be preferred. We need not find fault with these arguments as they have been put forward up to the present hour, because there has often been fair reasoning in them, even when they have seemed most clearly the mere voice of convenience or self-interest. I have stated myself that it is better to drain a town into the sea than let the sewage accumulate

in the town. I have been summoned to inspect a sewage farm near to a large town, and have been obliged to admit that, correct as this disposition of the sewage is, on the principle of the sewage to the land, the water to the river, the experiment was not altogether the establishment of a garden of roses or a paradise of health. And, again, I have, as already intimated, been compelled to recommend the cesspool rather than make no recommendation at all. Once I remember being so placed as to have to utilize the earth closet system, and with far better results than could have been expected. But, in every instance, there has been nothing except temporary necessity as the propelling action.

A very distinguished engineer, one who has made drainage a special subject of practical study, and who, indeed, has drained a large place most satisfactorily as things go, told me, quite recently, that although he was entirely in favour of the separate system of drainage, and of the utilization of the sewage for agricultural or horticultural purposes, yet he was not able always to enforce that system, because the exigencies of practice sometimes demanded a compromise. It is unnecessary to dispute this position under existing conditions.

Recognising all the difficulties, and submitting to the local necessities with the best grace possible, I have kept it in mind for over thirty years to try and find the way to some system of unity that should make it possible for drainage to come into such uniformity of method that every place should accept the adoption of the same method. If this could be done, then we might, in the end, secure a truly national system; and as any system, if it be right in principle, however crude it may be at first, admits of steady improvement as it goes on being developed, so I determined to undertake an exposition

of the most elementary system of the kind, should it occur to me, and should it promise, in the course of development, to be a success. In this line of study many suggestions have offered themselves, some of which I have ventured to put forward. In my model city, Hygeia, I sketched out a plan for the removal of the sewage, which our late President and the late Dr. William Farr greatly approved of, and which, in so far as the arrangements within the city were concerned, were all fairly good, so good that in regard to mere local plans one would not wish to change them. But then the plans were purely local; they went no further than the city, and for ultimate distribution of the sewage they left too much to necessity due to local difficulties.

Suggestion for Floating Tank Reservoirs.

For the drainage of towns on the sea-shore I once suggested a simple and perhaps effective method. I proposed that the sewage should be drawn from the town by exhaust pumps into floating tanks lying in docks by the shore, and that when the tank was laden it should be tugged out to sea at a long distance from the shore, and emptied into the sea by opening the tank from the keel, in the same manner as in the barges which are in use in rivers where the process of deepening is in progress. Or, that, where it was convenient, the laden tank should be towed to parts of the shore where sewage was wanted for agricultural or horticultural purposes, and should be unloaded there. A series of tanks thus brought into action might enable the largest seacoast towns to be cleared of their sewage completely, and with an exhaust system so perfect that, whenever a closet or sink were opened to let new sewage flow down it, a current of air should also be drawn down it, carrying

all bad odours with it, instead of allowing such odours to diffuse back into the dwelling as they now do. In one instance I nearly succeeded in getting this plan tried, and estimates were obtained for the construction of the floating tanks; but an unexpected opposition intervened, and I am now glad that it did, since the adoption of the method, if it had proved successful, might have been, as I now think, not exactly a retrograde step, but a step that would have impeded a more comprehensive and better system. The fault of it would have been that, if it had answered mechanically, it would have been imperfect for utilization, since the temptation to pour all the sewage into the sea would have been irresistible.

I thought of extending the same plan to towns that were placed near to rivers or canals, and before the main drainage system of London was started I wrote a letter of suggestion to this effect, to Lord Derby. The proposition was that along the Thames there should be constructed a series of intercepting tanks or floating sewers, which could be charged day by day, without in any degree polluting the water of the Thames, and that when charged they should be towed out to sea, and emptied into the sea, or towed to some out-of-the-way part of the coast, and there discharged of their contents, for purposes of utilization. I have never felt any doubt that if the plan thus proposed had been carried into effect, in combination with the new lines of sewers that were laid down, but so modified in convergence as to make the floating receptacles the terminals, there would have been a far more effective system for sewage removal from London than that which now exists; but for the reasons already stated we cannot think it would have been a perfect system, or one supplying the requirement of unity. It would have been considered applicable to the metropolis alone,

and it would, for sheer conveniency, have led to the loss of all the valuable products of sewage, a loss to be more and more deplored as day by day passes by.

Plan of Requirements for National Drainage.

I drew out a plan of requirements for a national scheme as the basis of further study, and for the heads of this plan, as necessary to success, I wrote down the following.

(*a*) The first essential in the plan must be utilization of all sewage products, utilization either on the farm or the garden. This stands first, as in the strict order of nature.

(*b*) The second essential is the entire separation of storm water from sewage, with limitation of water to the lowest possible degree for conveyance, so that the sewage may not be rendered putrid and useless in its transit; with diversion of the storm water, by distinctly separate channels, either to pure watercourses, or, in large towns, to reservoirs, where it may be applied to domestic purposes, for purification of the body or of the garments which cover the body.

(*c*) Such an arrangement that the sewage shall be removed from every dwelling and every town as rapidly as it is produced.

(*d*) A method which shall convey all sewage matter so cleanly and quickly from human habitations, that there shall be no necessity for ventilation by openings in public thoroughfares, and none whatever in connection with private houses or other buildings.

(*e*) An exhaust method, in operation at every minute of the day and night, for flushing the sewers with a down draught of air, as well as of water, whenever a closet or sink is opened, in order to allow the substances and fluids put into it to flow away.

(*f*) Lastly, one system that shall be universally applic-

able, and so simply applicable that all communities shall be able at once to avail themselves of it; and that shall be invariably introduced as a necessary part of every design and plan for new towns as they are originally laid out by the builder. In short, a scheme of national main drainage as distinguished from purely local schemes and from real or assumed local necessities.

NEW PLAN FOR NATIONAL MAIN DRAINAGE.

A year ago a plan occurred to me that seemed absolutely practical for carrying out every one of the requirements above stated, but I have been in no hurry to publish it. I have thought it best to think it well over before saying a word about it to any one, and to let it fall to the ground without another word if, on reflection for twelve months, it lost the force with which it originally appealed to my mind. It has not lost its force, but has become more forcible as it has been more and more considered; and it is to this design, under the title of "National Main Drainage," I am now about to invite attention.

It has appeared to me, as in a map of national drainage, applicable on the widest scale, that the whole country is already surveyed, laid out, levelled, and prepared for national drainage by what has, with infinite toil, engineering skill, and scientific design, been accomplished in the network of railways with which the country is everywhere intersected.

It struck me that we have now nothing to do but along the sides of all our lines of railway construct a series of tunnels, either in iron tubing, earthenware tubing, or brickwork, as may locally be most appropriate; to let this main conduit, or sewer, start at the commencement of every place where there are houses that require to be

drained and that lie near to it; and to let the sewage which is pumped into it make its way, in the main courses, by the side of the line, or, if necessary here and there, beneath the line. Then, in its course, at proper stations in country districts, to let the sewage be collected and conveyed by other conduits to spots selected for its utilization, so that the land all over the country may receive the benefit of it for fertilization, away altogether from the residences of men and in a manner perfectly harmless to the health of communities.

Points of Detail.

Such, in a broad view, expressed in as few words as possible, is the scheme proposed. Let me now pass to a consideration of details, which sometimes are apt to destroy the best laid-out general plans, but which, in the present instance, give satisfactory support.

The first point of detail is, that the levels are laid for the plan, and that sites already exist for the necessary constructions. There is such ample open and unused space for the mains, it would be no more difficult to lay them down than it has been to lay out telegraph lines. No injury to the railway could occur, and considering how wide would be the area of distribution, it is surprising, when we come to reckon up, how small the conducting tubes or conduits, taken as a whole, may be. In the immediate neighbourhoods of large towns, the diameter of the conduits would perhaps be measured by a few feet, but along the lines, for hundreds of miles, they would taper down to inches, and would often cease altogether for long distances. In the case of the largest towns, like London itself, there would be so many starting-points that there need never be enormous ingoing sewers. Every district would drain into its own main, that main would run into

the outgoing nearest line, and, as the contents would always be in brisk current outwards towards the open country, with a minimum quantity of water to carry the solid material, size, even in such a vast city as London, would be, by comparison of present sewerage, extremely small. The sewage also of such a vast community would be disposed of in every possible direction, on the principle of divide and conquer. Some hundred and fifty outlet stations could, I calculate, be placed within a radius of twenty miles of greater London, and, without any perceptible or possible nuisance, supply that wealth which, with perceptible and real nuisance, is now being cast into the sea.

The motor that would be required for the movement of the sewage along the conduits would be of the simplest character. The gradients would be so slight that the pumping power demanded would be comparatively inexpensive for each inlet from a city, town, or village. At proper points there would be a pumping station, which by exhaust would pull the sewage from the town, and by return stroke would raise it to fall to the level in the direction leading towards the exit or exits. As the main tube would be easy of access at any point, it would be possible to tap it, so to speak, at any point along the line, in order to draw off the contents for utilization; and, in numerous places, this could be so arranged by the selection of outlets, that nothing more than gravitation through the distributing tubes would be required. In passing along a line of railway, we see constantly on either side great wastes of country, lying below the level of the line, the barrenness of which tells its own tale, tells that it demands for new life that which we have so long been casting away. In such neglected spots as these, a few hundred yards of supply tubing would transform sterility into fertility, and convert the waste or moor into the fruitful field. I

observed the other day, in travelling down the South-Eastern line, no fewer than twenty-one such spots, over which, without injury or unpleasantness to any one, the sewage of the different towns on the line could be fruitfully dispensed, and without any motor power being required to distribute it save the natural power, the attraction of gravity. Constantly, too, it may be observed that large slopes of land and angles belonging to the railway itself are bare simply from want of fertilizing food. All these spaces might be turned into orchards, fruit gardens, or flower gardens with facility.

In many country places water-power might be utilized both for pumping the sewage out of the inhabited sites, and for distributing it afterwards to the soil. In other instances, wind-power could be employed for the same purpose, a fact which will be recognised by every one who is old enough to remember with what efficiency wind-power has been applied to the drainage of marshy and fenny districts, and that too with mills of the most indifferent construction. In all instances, the wind-power, where it was applicable, could be used for distribution, as well as for the collection of the sewage matter; that is to say, the wind-power might be used, at favourable times, for charging with sewage from the main a raised tank, from which stored supply the after-distribution to the soil could be effected, at convenient seasons, by gravitation. Where neither wind-power nor water-power was available, engine-power could be called into play in all the agricultural districts, by the use of the traction-engine used for ploughing land, and now in such common use that we meet it almost daily in our country roads. This engine, also, stationed at one or other of the sources of supply from the main line, could readily be adapted to serve the purpose of tanking and distributing sewage over the

whole of an estate. In course of time, farms in the neighbourhood of the railway tracks would enter into arrangements to have a supply sewage tube, or a series of tubes, laid along their estates, from different points of which they would draw off, from the main, the charge of sewage they required for the different parts of their estate.

Removal of Drainage from Water Supply.

You will not be slow to observe that, if this plan of removing all the sewage from habitable sites, and giving it up to the land, were carried out on the extensive and natural method I have suggested, the whole question of pure water supply would be at once settled in a health-giving manner. Under this plan, every good supply of water from lake, river, and rivulet could be utilized for domestic purposes, without being polluted with sewage or becoming the bearer of it. Storm water would feed the river, sewage would feed the land. Each village and town would have its separate water supply, and great epidemics of disease carried by water would cease. We have seen, by rude accident as it were, wholesale safety springing from such protection in towns not by any means famous for their drainage and for their sanitary soundness in general. The town of King's Lynn occurs to me at this moment in illustration. In the year 1832 cholera, then widespread, and attacking places all round Lynn, passed by that place altogether. In 1849 it did practically the same, and in 1854 the fact was repeated, although the disease was actually imported into the town. In itself Lynn was cholera proof. The immunity was not due to the drainage, for up to the year 1821 drainage was thought to be next to impossible, and after that time it was most imperfect. When in 1864 I visited the town to write

a report on it in its medical and sanitary aspects, the condition of things was lamentable. Surface drainage had not been done away with, and portions of the town provided with trapped drains lay on the margins of three "fleets," or, in other words, open sewers into which the whole of the sewage from the houses was discharged. I reported that when the tide was out the "fleets," which were in connection with a tidal stream, were most disgusting. They formed a kind of network of open sewer, and were so wide and deep that bridges were built over them for the purposes of traffic, and boats could go up them when there was a sufficient draught of water. Looking down into one of these fleets, called the "Purfleet," I witnessed at low water the discharge of six closets down the steep of the open sewer, while every kind of abomination lay at the bottom, with small boats stranded in the midst. The sewage was at that time only movable by the tidal current at ebb and flow, and finally passed into the river. There was here presented a truly striking example of one of the worst-drained towns of the time specified; nevertheless, it escaped, signally, the worst of the drain plagues, Asiatic cholera. Why did it escape? It escaped because its water supply was removed entirely from the surrounding pollution. In its early days its water supply had been obtained with the utmost difficulty; but in the year 1423, in the reign of Henry VI., its Mayor and commonalty had the common sense to cut a canal from a place near the town called "Kettle Mills," up to a river known as the "Gaywood." The Gaywood rises from three springs seven miles from the town, and one can scarcely imagine a better source. In the time of the Commonwealth the water supply was improved, the river was cleansed, and the amount of water brought down was rendered more abundant. Wood was fetched

from Norway, out of which waterpipes were constructed. A waterwheel was used with three pumps, and the water being raised to a great reservoir, the town was fed with water unpolluted with sewage. The result was what I have told. The water being pure, the diseases arising from impure water were confined to cases occurring in persons who were sufficiently imprudent to drink from the water of the ditches.

The application of this narrative is useful, though, for a moment, it be a digression from the direct line of my discourse. It is useful as conveying, by a great experimental proof, that under the plan I am now suggesting, of exhausting all the sewage of towns as it is formed, of keeping it away from the watercourses which supply communities, the immunity from diseases carried by water would be insured for the whole nation. If a town so badly drained as King's Lynn could, in its worst days, escape cholera, how much more surely would a town escape if its sewage were conveyed away as fast as it was produced, and were put to its natural use of conversion into wholesome grass, fruit, and vegetable substance, for man and lower animals? If all towns were so reformed; if we could warrant the accomplishment of this great achievement, not in one town alone, but throughout the length and breadth of the land, the results would be simply marvellous. They are results entirely at our command.

Simplicity of Detail.

A detail which adds distinctly to the promised success of the system proposed is, that it involves no necessity for an elaborate, expensive, or long-continued work in laying the main conduits that are wanted in order to

convey the sewage from the towns to its ultimate destination. The levels exist ready made, and the tubular arrangement merely requires to be laid on sleepers or posts, at the foot or on the slope of the inclines. I should not propose one large tunnel by the lines in any case, but a series of small ones, varied according to requirement. Probably tubes of iron of nine-inch bore would be sufficient everywhere immediately that a town has been passed. The tubes could be laid and hermetically sealed at each joint with great rapidity; and if one or two became insufficient, others could be added as called for. It would not necessarily mean that a line of tube or tubes should extend from one station to another. It would often happen that the tubes, from even a large town, could dispose of their burthen within a few miles of the town itself, and then nothing more would be called for until another town was reached. Thus the process of outlay could wait, in all cases, for demand. When a town gave notice that it was ready to be connected with the national main drainage system, the simple addition of another set of connections, with or without a new conduit, would be supplied without any preliminary expenditure for preparing the route.

As to towns themselves, there would be no more cost or trouble than now exists. It is true each town would have to be brought under one plan. It would have to adopt the separate system of laying down sewers from the buildings to the junction or junctions with the grand outlet or outlets; it would also have to adopt small mains, and lay these down thoroughly connected the one with the other, and with the still smaller house drains from which they were supplied. But as regards points of outlet, there would be abundant advantage, for this reason, that in all considerable places there would

be many positions in proximity to the line of railway, each of them leading to a ready communication with the main outlet and conduit. The town, therefore, could be drained at various points according to its necessities; if it divided into parts at different levels. If it consisted of a high town, a low town, and a middle town, for instance, each connection with the outgoing current could be planned to meet the situation without, perforce, bringing all the currents to one common centre within the place itself. Further, with the extension of a place, there could be, at once, an extension of the main drainage, by a new connection with the main, or with one of the principal conduits belonging to it. The adoption of this national system would, moreover, have this enormous advantage in the matter of economy: it would give rise to the moulding and manufacturing of uniform stock for drainage work. There would be one scale of size, one scale of price, and one scale of work. Hence cost of complete drainage, in respect to primary and permanent outlay, could be calculated to a nicety, and brought to a legal charge, freed of the endless disputation and litigation now so often displayed when drainage works on a large scale are introduced.*

It will be seen from the passages above cited that from off the main branches, by the side of the lines where the conduits are laid, the branching tubes should pass to supply the neighbouring soil for agricultural or horticultural purposes. There is yet another course open for development in a similar direction, which would

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* There would be another advantage, namely, that when the system of water supply of a place was known, the system of drainage and sewage would also be known, since the two systems, as my friend Mr. H. C. Stephens, M.P., has pointed out to me, ought to be equal.

be at once practical and profitable. It would sometimes happen that districts would be traversed in which the direct application of the sewage to the soil would be useless or impracticable; in such cases it would always be possible to find localities in which the sewage could be diverted to a centre, where it could be turned into the best guano, and afterwards sold in that form as manure. The manufacture of guano might be conducted quite far enough away to be unobjectionable, but still within easy reach of the line for ready transport by coal or luggage trains, so that the cost of transport would be reduced to a minimum. In some cases a single branch line might be carried from the main line to the works, with special trucks for the conveyance of the guano in its destination to the market or to the place where it was wanted for utilization. This mode of preparation would, I believe, be found so profitable that it might ultimately come into extensive application.

In a health point of view the only objection that could be taken to the plan of national main drainage here proposed is that along the line of railway there might be escape of sewage. This is a pure question of workmanship. With tubes well and properly laid, there would be no more chance of escape of sewage than there would be of gas from gas-pipes, or of water from water-pipes. At one time objection was taken to the conveyance of gas in railway trains because of the danger of escape; and, originally, the same objection was raised for laying down gas mains along the streets of towns. These objections have died away, as would also the minor one of laying hermetically-closed tubes for sewage along the lines of rails, at a distance from the rails themselves yet sufficiently accessible to admit of immediate repair should leakage, by any accident, take place.

Summary.

To sum up so far: the proposition, if carried out, would be, for a town, as follows. The present system of happy-go-lucky sewers,—sewers of one size in one part of the town, another size in another, their connection with houses here fairly perfect, there absolutely imperfect; with rain-water passing freely into one sewer and not into another; with openings from the sewers into the streets above, to let off the gases of decomposition; with drains running to the houses armed with a ventilating shaft, a manhole, and traps at the various important outlets; with a method so bad that, whenever a drain is opened at its mouth, a flood of foul air diffuses from it into the room or house in which it is situated; with water-flushing dependent on and varying with the seasons of the year for effective cleansing; with conversion frequently of the sewer into a sewer of deposit, terminating in some imperfect and untrustworthy outlet, and delivering up its charge to some useless or, it may easily be, some mischievous purpose, and, at its best, answering so badly that it has to be supplemented by cesspools, middens, or earth closets.—These failures, compromises, and follies of a false sewage system would be done away with altogether, to be supplanted by a system in which the drains and sewers would be each of uniform size and capacity, laid on a uniform plan; in which the drains from the houses would be connected with the sewers in unbroken continuation; in which all the uncertain conditions incident to the entrance of rain-water would be removed; in which openings between the sewers and the streets would be impossible and unnecessary; in which ventilating shafts from the drains into the air of the town would be unknown; in which the manhole would be

unnecessary; in which, when a closet, a drain, or a gully-hole were opened, a current of air would rush down instead of letting foul air stream out; in which cleansing by uncertain flushing would be supplanted by continuous exhaust; in which the sewage would be conveyed out of the place as fast as it as formed; in which the sewage so promptly removed would be immediately turned to a harmless and the most useful of purposes; and, in which all the increasing wants of the community would call for nothing supplementary, but by universal and practical extension from every living centre of the nation, from hamlet, village, town, and city, would bring the advantages of equal cleanliness, health, and economy.

If from the detailed outlines which I have indicated we turn our attention to the project as a whole, with a map of the railway system of the British Islands before us, we distinguish, at a glance, how splendidly, and I had almost said naturally, the whole country is laid out and prepared for the design. We give the old Romans great credit for their wonderful system of sewers, and any one who will read carefully my learned friend Alfred Haviland's paper, in the old *Journal of Public Health* for 1856, will not be anxious to take from those admirable engineers, and sanitarian engineers too, the meed of praise which is their just due. But in the presence of a system of sewage which would be commensurate with the universal drainage of this country, and by imitation would soon extend to other countries, including, first and foremost, France and the United States of America, the Roman work would dwindle into little compass indeed. Yet, after all, the extent of application is not everything; the kernel of the design is its simplicity, practicability, and, above all, its utility. By this plan London could be drained, with facility, from over one hundred and twenty points. The sewage

of East London would go for the fertilization of land from London to Gravesend, Rochester, Maidstone, and Ashford, and, passing round by Deal to Dover and Folkestone, would take up the sewage of all these and other towns in its route, and help to fertilize, from its own doors, if I may use the homely expression, the whole county of Kent, making that a garden such as it has never been before in the whole of its history. The system commencing on the North-East of London, and carried up to Cromer and Hunstanton, would fertilize by two lines some of the neediest parts of Essex, Norfolk, Suffolk, and a section of Cambridgeshire. In the opposite direction, the system of lines proceeding from South-Eastern and South London to Hastings, Eastbourne, Brighton, Portsmouth, and Southampton would help to fertilize Surrey, Sussex, and a portion of Hants. The system from the Northern side of the Metropolis, taking the course of the Great Northern line, would supply portions of Middlesex, Herts, Huntingdonshire, Leicestershire, Lincolnshire, Yorkshire, and on to the extreme north of England. The system connected with the North-Western and Midland lines would feed parts of Middlesex, Buckinghamshire, Hertfordshire, Oxfordshire, Bedfordshire, Leicestershire, Warwickshire, Northamptonshire, Nottinghamshire, Derbyshire, Staffordshire, Cheshire, Lancashire, Yorkshire, and on to Carlisle. The South-Western system, south of the Bristol Channel, would take in parts of Hampshire, Dorsetshire, Gloucestershire, Somersetshire, Wiltshire, Devonshire, Cornwall. On the side north of the Severn, the North-Western would supply parts of Herefordshire, Worcestershire, Shropshire, Cheshire, and the counties of Wales.

A map showing the railway lines of any part of the United Kingdom—or, for the matter of that, any part of the world where such lines exist—will indicate the

completeness of this drainage plan. In France, where the extremest differences of opinion are presented, where there is more perplexity and less advancement in practice than here, this plan would answer thoroughly. It would be carried out with facility, would be strongly in accord with the orderly genius of the French people, and would meet their agricultural necessities to the letter, since it would bring to their plots of land, large and small, that fertilizing material which they specially want, and would, I believe, quickly double the food-producing powers of their beautiful country, still, in essence, an agricultural country in the best sense of the word. In America, where the railway tracks pierce new and virgin lands—vast prairies waiting for cultivation—the system would also adapt itself with singular advantages. At little cost, thousands of miles of fruitful and beautiful gardens would soon fringe deeply the view of what are now wild and monotonous wastes. The railway track would bring food and fruit to the soil, and the railway track would carry that food and fruit to the peoples of all lands.

The Plan a Natural Plan.

"There is nothing new under the sun," said the wise Chaldean. He said truly. This project of mine is not new; it is only newly seen, for Dame Nature has planned it and acted upon it from the time that she placed living beings on her earth. It is a plan that is going on during each moment of our lives in our own bodies. The dark blood stream which is distilling every moment into our veins, from every centre of living action, is the impure but vital food flowing along the rivers of life. It is not food thrown away because it is impure: if it were we should quickly die from loss even of its impurity. No! the impure blood, after bartering with the air, is only reburied in our

tissues, in combination with oxygen derived from water and air; and, so revivified, it first replenishes and then returns impure, to be changed again and sustain the frame it animates. It is the same in respect to the reanimation of the mother earth. She yields us vital food as a loan, nothing more. She asks for it back when it has served its purpose, and revitalizing herself from it, under the same revivifying influences as are present in our own bodies, she restores it to us, if we will permit her, bringing forth from the very womb of death the most perfect forms of life.

National main drainage means the complete return of temporary dead matter to the living earth, over the widest possible area, and the application of temporary dead material for food. The accomplishment of this plan is by and through a capillary network laid out for another and different purpose, yet so well laid out for this very purpose, that if we were going to begin *de novo*, we could not begin better. That has been my subject for exposition. It has not been a theme so pleasant as the laying out of a model city, nor is it likely to prove as popular. I do not expect any sanitary enthusiast to republish this lecture, in a cheap edition, in combination with the "Pilgrim's Progress," as once happened to the more popular discourse. But in spite of all drawbacks, the practical nature of the present subject, and the urgent necessity that exists to take some immediate step in advance for securing perfect drainage, may enforce some attention and may incite some public bodies to take up the consideration of the project, for practical accomplishment.

The Organizations for Working Out the Plan.

These thoughts lead me to another question. If the scheme be practical, where should it commence? Who

are the persons to take the initiative in carrying it out? Is it a Government undertaking? If the Government had control of all the railways, then, in so far as the railway work is concerned, nothing could be better. Again, in regard to the local arrangements in each town. Is that a County Council undertaking? If the County Councils had control over the local drainage of every place in their district, nothing could be better. Unfortunately, neither the General nor the Local Governments have the control wanted, nor are they likely, in this country, to have it; they, therefore, must remain, for the time, out of the reckoning. We are thus left with the railway authorities and the local authorities for advancement. The happiest thing would be for the railway authorities to take the plan up as a thorough and profitable undertaking. If one of the companies, the Midland for instance, would set up one sewage station near to some town of moderate size, and, laying down a line of conduits, would remove and utilize the sewage of that single town, the whole difficulty would, in my opinion, be over. The working would be so natural and easy, and the results so immediately returning for good, that in a decade all the country would be completely drained, and all the sewage satisfactorily utilized. The whole engineering field lies open, with everything ready for the hand and skill of the engineer. Not one new instrument has to be invented; not one new line to be cut; not one tunnel to be made; not an acre, probably, of land to be bought, except for the outlying distributing or guano stations in agricultural districts; while every form of power—steam power, water power, wind power, electrical power—can be rendered available. At the same time the local draining of towns would be immensely simplified. The exhaust principle would do away with the present

elaborate and clumsy system of town sewers of large size; of water flushings from river or pumps; and, of falls in order to get draught. The small iron sewers need not lie lower in the earth than the gas mains now lie, and, being easily got at, would be readily kept intact, and readily relaid or repaired. One or two intercepting sewers of larger diameter, and running towards the railway pumping station, would completely connect the whole, and bring the full current of dead matter, free from decomposition, out of the town as fast as it was produced, and into its proper channels for removal to the soil.

There is another resource. Supposing that the railway companies did not see their way to take the initiative, and supposing that the local authorities of towns did not see their way to co-operate with railway companies disposed on their part to entertain the project, might not some intermediate public body be formed, on sound principles, that would arrange with the railway authorities for lines of transit, and undertake the drainage of localities, with utilization of the sewage, on the plan laid down? Such an organization might, no doubt, be formed, and in it lies a vast field for employment of useful labour, and for returns that would richly repay the outlay, to say nothing of the national profit that would be earned by the brain and hands of such an organization at work not only in England but very soon wanted abroad; with a new industry created which, like the railway system itself, would be English to the backbone in its details and in its developments. I do not dream on this matter. I am not enthusiastic. I speak the words of truth and soberness; and, strange as it may seem to some because of its novelty, I am sure I speak in advance of what must come. Necessity knows no

law, and in this country necessity for food, which will not for ever be satisfied from abroad, and need not now be fetched from abroad if it were duly cultivated at home, will force us back to our own fair fields for our staffs of life and stamina of health, and will enforce the formation of a special organization, if existing ones do not take the initiative, as we may hope they will.

National Pure Water.

One word more, before I conclude, as a stimulus to the railway companies. Let them remember that a service of transit that can bear away can also bring. Let them issue return tickets for sustaining supplies. All our towns and cities want pure water, brought from pure lake and spring. The network of railways, with their exquisite levels, intersects every source of water supply. Haply, therefore, if on one side of their lines the railway companies should successfully remove the sewage from villages and towns, they would be called upon to bring down by the other side of their lines pure water, and be paid for the return ticket in terms agreeable to their customers and profitable to themselves. But this only in the way of a seductive ideal. For the moment, I wish to let national main drainage be the one and dominant consideration.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

POST HOC, ET PROPTER HOC.

AS the physician grows old in practice he remembers such anomalous experiences that the dogmatic beliefs of his earlier years get many a shake and shock. In the close of last year, during the most inclement weather I ever remember to have experienced, I had a neighbour under my care suffering from pneumonia in the base of the right lung, coupled with bronchial irritation and an occasional pleuritic rub, with pleuritic pain. The fever was at 104° for three days, and altogether there was cause for serious anxiety. I treated *secundum artem*. I had the temperature of the sick-room kept steadily at 64° Fahr.; an excellent nurse was in constant attendance; and, not to be tedious, under this and other treatment the sick man made a good and, comparatively, rapid recovery. Here, said I, is the value of firm and protective treatment; here is the success of modern medical art. But whilst I was still congratulating myself on this triumph of skill, there appeared one morning in my consulting room an old patient and old friend, physically a much weaker man

than the other patient, complaining of having taken a severe chill, of pain, anteriorly, in the lower part of the right lung, of cough, and of difficult breathing. He had painted his skin, over the region of pain, with iodine, and when I turned my stethoscope to the spot, I discovered, to my surprise, a considerable area of fine crepitation and a sharp friction rub, pleuritic in character, rendering the breathing very painful from "stitch." Here, again, also was bronchial irritation; and, in short, all the physical signs of broncho-pneumonia, passing into the second stage, with pleurisy. There was this difference, however, betwixt the present patient and the first, that the temperature was but one degree above the normal; he had been very much more feverish, he reported, "but he had walked it off." I had a man to deal with of tremendous will. The day was one of the bleakest, coldest, most forbidding of all the series of bad days in the season, but this was not a man who would lay up; once begin to lay up, and he would simply "lay down for good." He meant to keep on, and he did. I prescribed as best I could; I put round his chest a flannel bandage, and in every way met the circumstances as well as I was able, not at all sparing admonition. Three days later he reappeared, still ailing, but improving. The pain was less, the breathing freer, the cough looser and easier, the appetite better. He had lived in his usual manner, and had only varied from his ordinary courses in that he had not kept at work. When he felt feverish and oppressed he walked those symptoms off; and in ten days he had recovered entirely.

I notice the facts of this case, not to say that I recommend the course pursued, but to utilize the observation. Is it possible that the patient was, without

knowing why, following a sound line of physiological treatment? Did he, by exercise, reduce febrile action? And, might we venture to bring active muscular exercise into play in the treatment of acute pyrexia? It is a suggestive idea. But the great fact is the fact of recovery from serious and well-defined symptoms under such circumstances. If this gentleman had held a prominent public position, how many consultations would there have been, how many bulletins, how many reports of details of the pleurisy and the pneumonia? What number of days or weeks would he have been told to keep the house at the risk of his life; what the number of speculations as to the place on the Continent, or elsewhere, to which he must migrate till the winter was over and gone?

It makes the history of my friend and patient more important if we recall that his case is not, after all, singular. In the post-mortem room we find constantly pleural and other lesions which must, at the time they were existent, have caused considerable ailment, of which there had not been a suspicion. The Father of medicine was right. The "*experientia fallax*" holds too often first place.

STOMACHIC TOOTHACHE.

WHAT is the physiological pathology of simple stomachic toothache? It occurs in persons who have decayed teeth, and it occurs in persons who have mere stumps of teeth, stumps which seem, to all intents, dead. As a rule, it affects one decayed tooth or stump alone, although there may be others in the same condition of decay. The ache, when it is on, is one of the most agonizing that can be

experienced, intensifying rapidly, and reaching sometimes a pitch that seems for seconds relieved by its very acuteness, as if the vibrations were too rapid to affect the sensorium, like a whistle out of sound by its actual intensity. All the while there is dyspepsia; flatulency in the stomach and intestines, with much uneasiness, without acute pain, but with irritability of the kidney, and passage, probably, of pale urine. Suddenly there is an eructation, attended, usually, with rising of an acid fluid into the mouth, and followed, often instantaneously, with entire cessation of the local ache in the tooth, however sharp it has been. The reprieve may be prolonged for hours, or may be stayed altogether, if the dyspeptic symptoms do not return.

These are the phenomena of dyspeptic toothache. The trouble, to my mind, is to account for them. To call them dyspeptic is merely to use a phrase which connects two sets of symptoms correctly enough, but explains nothing more. Is it that there is a direct nervous connection or easy indirect communication between the nerves of the stomach and the maxillary line of nerve? Or is it that an acid secretion in the stomach extends over the whole of the mucous tract, and, from the buccal surface, finds its way into the open cavity containing the end of the exposed nerve, which it subjects to irritation? From the fact that the liberal application of an alkali to the aching tooth will sometimes at once relieve the ache, the idea of an acid irritant is not without support, and is, I believe, half correct; that is to say, in some instances, an acidity concentrate in the stomach, is diffused over the mucous tract. But the fact of the sudden relief on the occurrence of eructation gives proof that the irritation is nervous from the stomach, and is more than a mere

local irritation of the nerve at its terminal point. Supposing this to be true, it must be admitted that the nerves of the other teeth are subjected to the same irritation, from the common centre of irritation, and still do not ache: a fact showing the presence of another element than irritation or increased vibration; and that other element is, clearly, exposure of the extremity of the nerve. The nerve, so to speak, is left free to vibrate, with the result of pain, from communication of rapid and intense vibration to the sensorium. But the primary cause of the ache is due, I believe, to dyspepsia induced by feasting on rich and saccharine foods and drinks, at times when the body is not allowed a free amount of exercise, or when it is confined too closely indoors. The treatment, therefore, must be general as well as local. The local and immediate treatment consists in applying carbonate of soda freely to the aching tooth, keeping the soda for some time in good contact, and then, after drying the cavity thoroughly, inserting into it, so as to reach the nerve, a concentrated solution of carbolic acid. This allays the local suffering, temporarily, but it requires to be coupled with the general measures,—a few doses of bicarbonate of potassa, a brisk mercurial purge, a warm or Turkish bath, and vigorous out-door exercise.

WINTER RESIDENCE OUT OF ENGLAND.

PATIENTS and others who have the means to winter out of England, and to take three or four months' residence abroad, often consult the physician on the question of the best time for this change. The custom is to leave early in November, and to return home in

February; but it is not sound medical ruling to sanction that custom. November, by comparison with the spring months in this country, is a favourable month, and December is not bad; added to which, the months immediately following autumn afford a period when the body, largely recuperated, is able to bear, fairly well, the succeeding two to three months of cold. It is, consequently, the best advice for the ailing who desire to go abroad, and who have only three or four months to utilize in this way, to recommend them to winter in England until near upon Christmas, and, if the weather be good, until after Christmas, and then take their vacation in the warmer climate until April. This plan has two reasons for its adoption. It causes the body to revivify as the autumn restoration loses its effect, and it protects the body through the most trying months of the English year, namely, January, February, and March, the three months during which most fuel is demanded for the house, and in which the animal fires of the enfeebled go out most readily. Plainly, three months in a warm climate, out of England, after Christmas is, *cæteris paribus*, well worth four months at any other time.

CANCER AMONGST THE JEWS.

A STATEMENT has been circulated recently that the "chosen people" are of all people most exempt from cancer. It has been assumed, also, that this exemption is due to the peculiar mode of diet they follow, their avoidance of swine's flesh, and the excellent care they take against the use, as food, of animals that have shown in

their carcasses any indications of disease. The care exercised by the Jews in avoiding unhealthy animal foods is most praiseworthy, and in lecturing on foods I have many times dwelt on the fact; but I regret to be obliged to state, from personal knowledge, that the Jewish community is by no means exempt from cancer. I have known the disease to occur, and prove fatal, in several members of their community who were not under my professional care; and I am obliged to recall that I have had under my professional care others of their community who have died from the disease. Neither is there any difference in the character of the affection in the Jew, any special selection of sex, any organic selection. I think that with the Jews, as with persons of other races, women are most liable to cancer; but I have witnessed the affection in both sexes. One Jewish patient, at least, whom I have known professionally, has died from malignant stricture of the œsophagus; two, at least, of Jewish women from uterine cancer, three from cancer of the breast; two —one a man, the other a woman—from cancer affecting the lower part of the alimentary canal; and one from cancer of the liver and stomach. These are not all my unhappy experiences on the subject; but they are amply sufficient to disprove the rash assertion of racial exemption from cancer. If phthisis pulmonalis had been named instead of cancer, I should have said there was some possible measure of truth in the assertion, for although I have had Jewish patients under observation for phthisis, they have been much fewer, proportionately, than patients of other races. In hospital practice I found this comparative freedom from phthisis remarkable, though not absolute.

THE AUTOMATIC SYRINGE.

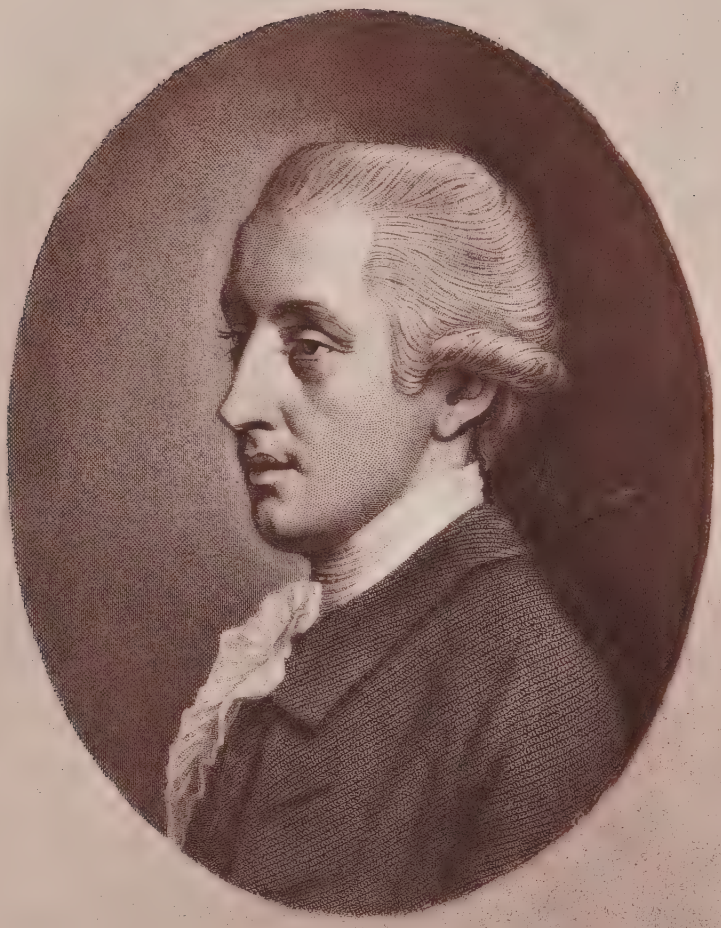
THE automatic syringe described in my paper, read in March last before the Medical Society of London, and published in the *Lancet* of April, is of general application. It can be used after the manner of an ordinary syringe, and with a slight movement can be made to act as an aspirator as well as for injecting a fluid into the cellular tissue or into a cavity of the body. The speciality of the syringe, if I may so express it, is that it is always in work when once it is put into action. The operator has simply to move the piston up and down to fill and empty at pleasure, without any unlimbering of the parts, removal of the needle from its place, or the trouble of undoing the nozzle to refill. The size of the syringe need not vary, because, as a continuous injection can be sustained by it, the operator, by keeping it at work steadily, can throw any amount of fluid into the part he wishes to inject, in a very short period of time. Into a cavity, if there be no great opposition, half an ounce of fluid can be injected per second, or thirty ounces per minute; which really means that an instrument of one half ounce capacity is capable of fulfilling almost all the service of any operation, from the injection of a single dose of morphia to the administration of some pints of fluid as an enema or transfusion. Messrs. Krohne and Sesemann have now fitted up the syringe with sets of needles of different lengths and diameters, for various kinds of needle injections, and with a nozzle that will take a needle, the end of an injecting tube, the end of a flexible catheter, or any other tube required, either for injection or for aspiration.

WILLIAM HEWSON, F.R.S.

AMONGST its other treasures, the Medical Society of London possesses a famous picture painted by S. Medley, representing that quaint old Quaker physician, Dr. Lettsom, of doggerel fame, if such be fame:—

> "When people's ill they comes to I;
> I physicks, bleeds, and sweats 'em.
> Sometimes they live; sometimes they die;
> What's that to I? I Lettsom,"—

presenting a gift to the Society. In the chair, raised above the rest of his Fellows, is Dr. Sims, the President, himself a man of learning and of antiquarian taste, to whom the Society is indebted for some presents of considerable value. Sims, as the presiding genius, wears the cocked hat; and around him, or rather before and around him, sit the great men of his day, men whose names, figuring largely in their time, have passed now into the shades of the forgotten. Some time early in the fifties, I remember Mr. Medley, still hale in the flesh, coming down to the Society's rooms to look at his picture, which he had not seen for over half a century. The then President of the Society, John Bishop, F.R.S., with Dr. Chowne and a few others of us, met him, and heard from him as many details as a man of perhaps ninety summers could call to mind. He named some of



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William Hewson.

Autotype of an engraving by
H. Robinson, first published
in Gulliver's Life of Hewson, from
a picture by Vandergucht.
B.W.R.

the men who had once sat to him, but amongst all the figures on his canvas, one in the background seemed to interest him most; a fact the more peculiar because it was a picture of a bust, not of an actual figure. "I have heard that that was a clever young man," he said, "who died early, before I came on the scene, but I forget his name." The gap was filled by Mr. Bishop, who at once remembered the name "William Hewson," a name we all recognised, as indeed that of a clever young man, the moment it was pronounced.

William Hewson was, however, something more than "a clever young man." He was a man of industry, honesty, and original observation. Had he lived long enough, he might have rivalled John Hunter himself, whose cotemporary he was. There is not much to be told as to the life of William Hewson; it was a life too short and too concentrated on one object to admit of much history: but respecting the work of his life a great deal may well be said, for it was work of the highest order; work much neglected in the present day in favour of what is infinitely less true and valuable; work that cannot be put out of sight or out of memory, but will rise again and again, and declare itself in all its truth and simplicity. This also attaches to the work, that its author died at it and from it. When a soldier of mark dies in battle from a wound, we make a great deal out of the event; we signalize a hero, and write songs and essays in his honour; while we let the man of science, who may die from a wound in the battle of science, go to his grave without so much as a thought. A commonplace death, as it is conceived, but really as noble a self-sacrifice, and may be as heroic, as that of any soldier who has fallen in any field of vanquishment or victory.

In a few pages, the late Mr. George Gulliver, himself

one of the broadest and honestest of surgeons, has condensed the leading facts known of our present scholar, and in his work and collection of the essays of Hewson, for the Sydenham Society, I find ready to hand most of the leading facts that can be obtained. Chiefly from Gulliver's book, therefore,—which I prize the more because it is a gift from him, with one of his kindly expressions on the fly-leaf,—let me sketch, in a few lines, what can be said of a biographical nature.

PERSONAL HISTORY.

Hewson was eleven years younger than John Hunter. He was born on November 14th (old style), in the year 1739. He was, so to say, born in physic, for his father was a practitioner of medicine at Hexham, in Northumberland. His mother, whose maiden name was Heron, was well connected. He was one of eleven children, all of whom, except three sisters, died before he died. After the usual preliminary education at the grammar school at Hexham, under the Rev. Mr. Brown, he began to study medicine with his father. Then, in order to improve his knowledge, he went to Newcastle-on-Tyne, where he became the pupil of Mr. Lambert, a surgeon there of considerable repute, and, in fact, in his day, as I have learned, the leading surgical authority in that part of England. From Mr. Lambert's practical teaching he came to London, from London he passed to Edinburgh, and from Edinburgh to Paris, adding, in the famous schools of these cities, to his stores of information theoretical and practical. We gather these brief facts from a letter which, after his death, his widow wrote concerning him to Dr. Samuel Foart Simmons, the biographer of William Hunter. She became, she says, acquainted with him in the year 1768. She was a personal friend of the

famous Benjamin Franklin, who was in London at this time, lodging in the house of her mother, Mrs. Stevenson, and her narrative is too simple and touching to be omitted. "He" (Hewson) "was at that time in partnership with Dr. Hunter. Some similarity in our dispositions created a mutual esteem, and the equality of our situations made our union desirable in point of prudence. I had five months the start of him in age, no pretensions to beauty, nor any splendid fortune; yet I believe he was satisfied with the choice he made. We were married July 10th, 1770. I brought him two sons. The elder was just three years old when Mr. Hewson died, which was on May 1st, 1774, and I was delivered of a daughter on August 9th following. His last moments of recollection were embittered by the idea of leaving me with three children scantily provided for. The trial of my fortitude was different; the loss of affluence I did not feel for myself, and I thought I could bring up my children not to want it. However, by the death of an aunt, who left me all her fortune, I became reinstated in easy circumstances, and enabled to give a liberal education to my children, who I hope will prove worthy of the stock from which they grew, and do honour to the name of Hewson."

Continuing her narrative, written in 1782, Mrs. Hewson adds: "Mr. Hewson's mother is still living at Hexham, and has one daughter, the youngest and only remaining child of eleven. His father died in 1767, and having had so large a family, it will be readily supposed he could not give much to his son, so that Mr. Hewson's advancement in life was owing to his own industry."

His professional studies completed, William Hewson came to London, as the centre in which he was destined to cast his lot, and was naturally attracted to the school in which William and John Hunter taught. He lived at

first in John Hunter's house, commencing his studies in the autumn of the year 1759. At that period, as will be recalled by those who are conversant with the lives of the brothers Hunter, John Hunter was in precarious health, and was advised to go abroad; advice he followed in 1761, by which time he had won such favourable notice from William Hunter that he succeeded John as anatomical demonstrator in the school. As a teacher, he had talents which John Hunter had not; he seems to have been remarkably gifted in power of exposition, and he began at once to earn his living by his tutorial skill. He was not, however, content to rest on his oars: he began to study at Guy's and St. Thomas's; took out-classes in midwifery of Dr. Colin Mackenzie, and in physic of Dr. Hugh Smith. In 1761 he went to Edinburgh for further study, carrying with him letters of recommendation from Sir John Pringle and from his distinguished master, William Hunter. In the winter of 1762 he returned to London, and recommencing to teach under William Hunter, finally went into partnership with him in an anatomical school in Litchfield Street, in which street he lived and received pupils in residence. John Hunter was still abroad, and when he returned to London in 1763 he found Hewson completely installed in his place in his brother's school. The fact does not seem to have caused any bad feeling between the two younger men, and John Hunter, independently disposed towards his brother's influence and patronage, simply started a school on his own account in friendly rivalry.

The partnership of Hewson with Dr. Hunter continued into the year 1771, and during that time Hewson made great professional and scientific progress. He visited, in the summer months of 1765, France, Flanders, and Holland, and in 1768, during the holiday time, he went

to the south coast, in Sussex, to make observations on fish, and prepare papers on the lymphatic systems of oviparous vertebrates. In 1769 he moved into the Windmill Street school of Dr. Hunter, where he had an apartment allotted to himself. In 1770, on March 8th, he was elected a Fellow of the Royal Society,—perhaps one of the youngest Fellows who had ever been admitted,—on the recommendation of the renowned Benjamin Franklin, Dr. William Hunter, Sir John Pringle, M. Maty, J. Turton, and James Ferguson; and on November 22nd of the same year he was awarded the Copley Medal for his papers on the lymphatic system in birds, amphibious animals, and fishes, and as the man who within the year 1769 had contributed most to the advancement of science and useful knowledge. At this period Dr. Benjamin Franklin was living in London, in the house of Mrs. Stevenson, a friend for whom he entertained much respect. Mrs. Stevenson possessed the daughter with whom Hewson fell in love, and whose simple and affectionate narrative has already been told from her own pen. His marriage with her took place after she had consulted Franklin upon it, on July 10th, 1770, and proved in every way happy and suitable. The young couple first lived in a house near to that of Dr. Hunter, but the year following some disagreement took place between Dr. Hunter and Mr. Hewson, which led to a dissolution of their partnership. The cause of the separation is not definitely recorded, but we may infer that Dr. Hunter was rather impetuous in his manner, and that Hewson resented something that was said or done by him. Franklin, as a "common friend," was appealed to by both, and during an "unpleasant time" was "obliged to hear their mutual complaints." The end of it was that the partners separated; Dr. Hunter continued to carry on the

Windmill Street school, and Hewson, having built for himself a lecture theatre adjoining a house in which he intended to reside, opened his classes: so that there were now two schools of anatomy branching out of William Hunter's school, the school of John Hunter, in Jermyn Street, and of William Hewson, in Craven Street, Strand.

A letter from Franklin to Dr. Hunter, in the possession of Mr. Hunter Baillie, and published by Mr. Gulliver, explains some part of the differences which led to the separation. Franklin refers to a memorandum of terms which he made between the two, dated August 23rd, 1771, in which it is stated that Dr. Hunter "expects Mr. Hewson should go on with his business as usual, during the remainder of the term they are to continue to proceed together, and during that time should make preparations at Dr. Hunter's expense, such as the Doctor should direct to be made, and others, so that those directed are not neglected nor omitted; that the preparations are to be the Doctor's sole property, and at his absolute disposal, so that if afterwards he should give any of them to any person (which he could not be understood to promise), such gift was to be considered as the sole effect of his good will." Later on Dr. Hunter presented, not to Hewson, but to Franklin, some specimens, which the latter, having no use for them, gave immediately to Hewson, believing that to be the intention of the donor. In justice to Hewson, Franklin adds "that his" (Hewson's) "conception of the original agreement between himself and Dr. Hunter, was that he had a right to make preparations for himself, the business and common interest of the partnership not being neglected." Here the two differed in opinion, but came to a kind of compromise, expressed in a paper which Franklin prepared and handed to both.

Before opening his new school, Hewson delivered, as

a preliminary, a lecture on the uses of the spleen and thymus gland, to a number of his *confrères* in the world of science. In his succeeding lectures to students he progressed well, getting together a large class, and adding, without seeking for them, the emoluments of the surgeon to those of the professor. Of his now promising career, his friend Lettsom recorded all that was most favourable: "Genius and industry were rewarded with success, and domestic duties with felicity. The theatre in which Hewson delivered his lectures and expounded his doctrines was crowded with men of science as well as with pupils, to listen to a youth grown sage by experimental researches."

Such was William Hewson when thirty-four years of age. Whatever angry feeling had existed between him and his old and famous master William Hunter, whatever the rivalry between him and active John Hunter, all was soon quieted, and was succeeded by a mutual desire to shine together in friendly contest, and in good will the one towards the other. In plain words, a brighter outlook never befell any of our fraternity than that of Hewson; he waited only for life in order to secure a brilliant course of activity, usefulness, and honour. The picture is pleasant to contemplate; and now, whilst our minds rest on it, let us look at the outcome; let us see what this bright and youthful genius, in the few years of his active industry, accomplished for the world of medicine in which he moved and had his being.

HEWSONIAN MEDICAL PHILOSOPHY.

I speak of Hewson's work as philosophy, and, if I have read his work aright, the one word "philosophy" is the only word that faithfully describes it. Gulliver admirably analyzes, annotates, and criticises the works,

adding largely to the history of every subject touched upon, and I recommend every one to read carefully what he says; but he seems to me to go into too much detail of commentary and minute dissertation. I will venture to look at the Hewsonian works as unity evolved out of details, which, though they need not be ignored, are secondary compared with the result that springs from them. Some men by bare industry become great, great by the mass of evidence they pile up for other men to build upon. Other men collect but little evidence, comparatively, and yet out of that collection construct so much that what they do is of moment in spite of its smallness. These last are artists as well as workers, and Hewson was conspicuously one of them; conspicuously so even beyond his two great rivals, the brothers Hunter themselves.

On Paracentesis Thoracis for Air in the Chest.

Hewson's first essay was on "The Operation for Paracentesis Thoracis for Air in the Chest; with some Remarks on the Emphysema, and on Wounds of the Lungs in General." The essay was written in 1766-7, and was communicated by Dr. William Hunter to "a Society of Physicians in London," from which emanated the well-known *Medical Observations and Enquiries*, referred to in previous biographies. The paper was read before this Society on June 15th, 1767, and was followed by a sharp contest between its author and the distinguished Monro Secundus, the Professor of Anatomy in the University of Edinburgh, with whom it was his (Hewson's) fate more than once to cross swords. The essay related to an improvement in surgery, namely, the operation of paracentesis thoracis in order to let the air out of the chest during emphysema caused by

puncture from a fracture of the ribs, or other wound in which the lung is punctured through the pleura. From the report of a case by Mr. Cheston, and from the dissection of a person who died after injury to the chest, Hewson came to the conclusion that the accumulation of air in the pleural cavity, and the confinement of it there, may occasion the worst symptoms, and even death itself. In illustration of this fact he relates cases from M. Littre, M. Mery, Dr. Hunter, and Mr. Cheston. In three of these cases the patients died, and it seemed to Hewson that the air confined in the chest was the principal cause of the symptoms,—the difficulty of breathing, the tightness of the chest, and the sense of suffocation, —and as these symptoms agreed with those of hydrops pectoris and of empyema, both of which are relieved by paracentesis thoracis, he was induced to believe that an operation for letting out the air might be performed with success. In order to determine what would be the effects of compressing the lungs with air from the pleural cavity, and then relieving the compression, he performed a series of experiments on the lower animals, which supported his views. He also made a *post mortem* of a man who had died after fracture of the ribs, which confirmed his opinion more strongly than ever. The result of his researches was, that in instances where the lung is punctured, whether by fractured ribs or other causes, whenever symptoms of tightness of breathing and suffocation come on, we must assign, as the cause, air pressure from the pleural cavity; and if upon this there be symptoms of extravasation of air into the subcutaneous cellular tissue of the body, as sometimes occurs, we ought to look upon that as a mode of relief, and either allow it free course or afford relief by puncturing the pleural cavity so as to give vent to

the enclosed air. The best place for performing the operation, if the disease is on the right side, will be the fore part of the chest, between the fifth and sixth ribs; "for there the integuments are thin, and in the case of air no depending drain is required." But if the disease be on the left side, it will be more advisable to make the opening between the seventh and eighth or eighth and ninth ribs, so as to be sure of avoiding the pericardium. A small puncture would be sufficient, "as the air requires not a large vent for its discharge;" and the best manner of performing the operation is with a knife, by a cautious dissection, rather than by the "coarse and hazardous method" of thrusting in a trocar.

The publication of the above-named essay, in the third volume of the *Medical Observations and Enquiries*, led quickly to the dispute I have alluded to between Mr. Hewson and Dr. Alexander Monro. In the winter of 1761 Hewson had attended Professor Monro's classes in Edinburgh, and the Professor now accused him of having advanced views which he had heard in class, and which had been expressed, originally, there. Monro seems to have been unmeasured in his terms of impeachment, and drew thereby a reply from Hewson, which has been published in full. In the reply Hewson says that, "though he may be as covetous of fame as most people, yet he is incapable of taking any unjustifiable methods of acquiring it;" and in rather a lingering, perhaps doubtful acknowledgment, he admits, from statements made to him by other pupils who had attended Dr. Monro's lectures and had taken notes of them, that he had been anticipated, but that at the time he prepared his essay he had never heard the least hint on the subject, either from Dr. Monro or any other person.

Monro, only half pleased, in declaring himself "sufficiently satisfied at having secured his title as the first who proposed that improvement," gave a rather pettish version of the whole affair, and still hinted that there was a little plagiarism after all, because a knife instead of a trocar was recommended by the younger disputant for the performance of the operation, as if the suggestion of a knife were a pretence at originality. For a moment the contention ended between the two authors, but it was never really settled between them.

Work Relating to the Lymphatic System.

In 1768 Hewson commenced to publish an account of his enquiries into the lymphatic system, his first production on this subject taking the form of a letter to Dr. William Hunter on the lymphatic system in birds. This letter was communicated to the Royal Society, and appears in the *Philosophical Transactions* in 1768, vol. lviii., pp. 217-26. In the following year—1769—he sent to the same learned Society a paper on the lymphatic system in amphibious animals, and later in the year a third on the lymphatic system in fish. The papers appear in the *Philosophical Transactions* for the year 1769, vol. lix., pp. 198-203 and 204-15. His researches on the lymphatic vessels and their contents were continued until 1774, when they, with new material, culminated in a treatise of experimental enquiries, containing a "Description of the Lymphatic System in the Human Subject" and in other animals, illustrated with plates; together with observations on the lymph and the changes which it undergoes in some diseases. The second part of these enquiries bears a dedication to Dr. Benjamin Franklin, to whom its author was much indebted for many acts of kindness and attention.

The works on the lymphatic system deserve study up to the present hour, and contain plates for illustration which also remain in some respects unsurpassed. A short history of the discoveries previously made on the lymphatic system is excellently put, and is memorable as containing a just tribute to the discoveries of Asellius, discoveries which supplemented and crowned those of William Harvey, but which Harvey himself, probably owing to advancing years, and that weariness for things new which creeps on with age, was inclined to ignore. Equal justice is done to Pecquet, Rudbeck, Jolyffe, and Bartholin; to Nuck, Ruysch, Glisson, and Meckel; to William Hunter and Monro; who by their labours had explained the anatomy of the lymphatic system, and the mode in which the lymphatic fluid finds its way into the blood.

Why Hewson pursued this same line of enquiry he explains to us in simple and forcible terms. When the lymphatic vessels were first seen and traced into the thoracic duct, it was, he tells us, natural for anatomists to suspect that as the lacteals opened into the intestines to absorb, so the lymphatic vessels—which are branches of the same system—might possibly do the same office with respect to the other parts of the body; and accordingly Dr. Glisson, who wrote in 1654, supposes these vessels to arise from cavities, and that their use is to absorb. Frederic Hoffmann also laid down the doctrine very explicitly, that the lymphatic vessels are a system of absorbents. Later anatomists, however, took a contrary view, and from experiments, particularly such as were made by injection, concluded that the lymphatic vessels do not arise from cavities and do not absorb, but merely are continued from small arteries. Hunter and Monro controverted this opinion, and, falling back on that of

Glisson and Hoffmann, endeavoured to prove that the lymphatic vessels are not continued from arteries, but are an independent system, and strictly absorbents.

It may seem strange to us in this day, and yet it is true, that anatomical investigation by the most skilful injection ever devised and practised, that of Ruysch, led to the opinion that the lymphatics run, continuously, from the arteries; yet such was the fact, and so strong was the opinion held on the point, that the illustrious Baron Haller himself favoured it, and contended that before the lymphatics could be accepted as absorbing vessels, it must be determined whether other animals, besides man and quadrupeds, have or have not this system. Hewson saw the force of this objection, and, setting himself at work to clear it up decisively, proved the affirmative by discovering the system in birds, fish, and amphibious animals. In consequence of these discoveries, he also arrived at a knowledge of the existence of considerable varieties in the composition of those vessels through the various classes of animals; and by comparing the knowledge thus obtained with some facts he had observed concerning the blood, he was led to ascertain, as he believed, the use of the lymphatic glands of the thymus and of the spleen; structures "which have so long been considered the opprobria of anatomists."

An extreme and yet natural enthusiasm, which riper years might have corrected, perhaps misled Hewson at this point; still we are bound to accredit him with much that is both original and practical in his work on the lymphatics and on the use of the lymphatic system. His anatomy is here shown to be of a high order, and his reasoning on physiological principles is equal to his knowledge of anatomy, although by necessity less solid as a foundation. We may accord to him the merit of

clearly showing that the lymphatic vessels are absorbent vessels; that the lacteal vessels and the lymphatics form parts of one system; and that the fluid secreted by this system is not a mere watery solution, as some before his time had supposed, but a coagulable fluid serving purposes in the economy beyond what is merely mechanical. Previous to Hewson it was surmised that the watery fluid present in the closed serous cavities is simply the condensed water which, when a cavity of an animal recently dead is laid open, escapes as vapour of water or steam; and on this matter he did not altogether differ from his predecessors. But he showed that, except in the watery fluid of the ventricles of the brain, the fluids named are coagulable; that they contain coagulable lymph; and, comparing this lymph with that found in the lymphatics, he asserted that it was of the same kind. Hence he drew the conclusion that the lymphatics, by absorbing the lymphatic fluid found in the cavities, prevent its accumulation and the establishment of that morbid condition known as dropsy. The brain is the only part exceptional to this rule; a fact he accounted for by the absence of lymphatic vessels in its structure, even in the choroid vascular network.

With much skill Hewson also indicated the process of absorption of poisonous substances by and through the lymphatics. More clearly than any predecessor, he pointed out that in a poisoned wound on the cutaneous surface, the red lines of an inflammatory character are lines of lymphatic vessels and lines of absorption. Most clearly he indicated that in the progress of such a disease as cancer, the absorption of morbid particles from the diseased structure to the lymphatic glands demonstrates absorption of material, which, in turn, leads to induration, enlargement, and disease of the glandular structure, with

dropsy in the cellular tissue of structures lying on the distal side of the glands affected. Very shrewdly he assigned to the lymphatic vessels the duty of draining the cellular tissue of its fluids; and with excellent argument he maintained that in the process of absorption an independent act is performed; that the lymphatic vessel is not a continuation of an arterial tube, as had been surmised, but is of distinct origin, whether it be considered as belonging either to the lacteal or the lymphatic system. It is perhaps not strictly fair to accredit Hewson, as some have done, with the discovery that in the thoracic duct lacteals and lymphatic vessels find their common centre, and that the final connection of the combined system is made with the veins at the point of conjunction of the thoracic duct with the left subclavian and jugular veins; but we may rightly accede that he described the courses of the absorbing vessels more distinctively than any one who had preceded him, and that he actually traced the lymphatic fluid from the thoracic duct into the venous current, and into the right auricle of the heart.

Like the brothers Hunter, however, Hewson made probably one original mistake on absorption. In his zeal to define the true action of the lymphatic vessels, he assigned to them too much. He made them appear as the exclusive absorbents, and denied all power of direct absorption by the veins. I cannot, after the most careful reading of his essays, come to a safe conclusion whether he doubted the absorption of simple water by the veins, and I infer that he did not; but he certainly denied the absorption by veins of all alimentary elements other than water, of all plastic materials, and of all that helps to make up solid tissue or organized fluid, while he assumed to the absorbent vessels every portion of that service. In this respect we now consider him to have been wrong,

but he was not without good argument for his hypothesis, and he was so just in argument, that had he seen that absorption may occur in vitalized structures where there are no lymphatics, as we have since seen, he would, without doubt, have accepted the demonstration in all good faith, with willingness of acceptance of the truth at all costs, and at all sacrifices of opinions previously expressed.

If in some way he went astray in the direction last named, he steered straight on and in the right course in another direction, from which his successors, up to our time, have widely diverged or kept away. That is to say, he studied carefully and with good promise the diseased conditions of the lymphatic and lacteal systems, and of the fluids which the lymphatic and lacteal vessels convey. He observed that in dogs in good health the lymphatic fluid always coagulates on being exposed to the air, and forms a strong jelly, and that in geese the fluid jellies sooner than it does in dogs; but that different conditions in the health of the animals make a difference in regard to coagulation. The change extends, simultaneously, in the fluids found in the serous cavities; a fact which confirmed his view that the serous fluids are identical with the lymphatic. Thus, in a dog which had subsisted for eight days on bread and water, and that sparingly, the lymph formed a very weak jelly, and in young geese the lymphatic fluid was later in gelatinizing than in such as are full grown. He also observed that when the fluid in a cavity is in larger quantity than natural, the coagulation is less firm, and slower; but in other instances, where the inflammatory condition prevails, the lymph, more viscid and more rapidly coagulable, forms, by its solidification, the layers and crusts which cover different surfaces of the body. Thus the outside of the heart and the inside of the pericardium are sometimes covered with a

crust as tough as the "size" of pleuritic blood, and the surface underneath has marks of inflammation, but is not ulcerated. Probably, therefore, he infers, it is the inflammation which produces that change, or which makes the exhalant arteries secrete a lymph with such increased disposition to coagulate. Add to this that the change which inflammation seems to produce is just the opposite to that produced by a dropsy; for in dropsy the fluid is secreted with an extraordinary quantity of water and too little coagulable matter; but in inflammations the fluid is secreted with a greater proportion of coagulable matter, and with less water.

An instructive contrast, which my own early experiments on coagulation fully confirmed, was made by Hewson between the coagulation of the fibrine of the blood and of the fluid of the lymphatics. He showed that the lymph, as he calls it, of the blood coagulates, under the same conditions, more rapidly than the lymph of the lymphatics. In a dog killed whilst in health he tied the veins and lymphatic vessels immediately after death. The blood in the veins had coagulated in six hours, but the lymph in the lymphatic vessels of the neck was perfectly fluid twenty hours after death; and, being let out at that time, jellied after remaining for some time exposed to the air. In another passage he speculates on the point whether pus itself is not lymph changed in its properties by passing through inflamed vessels. In one case he found three parts of pure pus in the pericardium, without any ulcer on that membrane or on the heart. More interesting still, he was acquainted with that condition of disease to which in these days we have given the name of myxœdema. "In this condition," he says, "the cellular membrane is filled with a gelatinous fluid, which does not ooze out when the integuments are scarified, nor does it

retain the impression on being pressed with the finger, as in common anasarca." This was remarkably evident in a woman who was in St. George's Hospital, and who had at the same time an obstruction of the menses, but no other symptom of ill-health. The legs in this woman were swelled, he says, to twice the ordinary size, but did not pit on being pressed by the finger.

A large amount of practical induction may be drawn from Hewson in this part of his studies. For example, in the treatment of cancer he dwells on the propriety of early operation before any gland is affected, if the operation is to be of service. He objects to operation when the glands are involved, unless the glands can also be extirpated; and, as everything that irritates the cutaneous surface is calculated to irritate gland structure, he objects to irritants and caustics for destroying malignant cutaneous tumours. He saw, too, in some instances of poisoning by absorption, as in the absorption of variolous and venereal poisons, that the toxic matter may pass into the body without touching a gland and without causing swelling or bubo.

It is painful to reflect how slowly those who have followed Hewson have been in seizing his admirable facts and suggestions on the influence of the lymphatic system in relation to disease. Of blood disease we hear plenty said, often, merely, as "words, words, words;" and of changes of the blood we hear sufficient; but how little about lymphatic diseases and changes of the fluids of the lymphatic system. Even in discussions on cancer we let the lymphatic system have the go-by, although the evidence is strong that it is the seat of the evil.

On the Functions of the Thymus and the Spleen.

After the death of Hewson, Magnus Falconar, another teacher of anatomy, collected and published his posthumous

essays on the lymphatic glands, the thymus gland, and the spleen. Respecting the thymus, Hewson taught that as the gland is largest in the earlier periods of life, and becomes gradually less as the body advances towards maturity, so that at puberty it practically ceases to exist, the inference to be naturally drawn is that the gland is necessary for the performance of some office requisite in the fœtal state, and in the early part of life depending upon the respiration. He looked upon the gland, in fact, as no more than a large lymphatic gland, and argued that in fœtal life it acts as an auxiliary to the lymphatic system, for the purpose of forming more of the central particles of the blood than could have been made by the lymphatic glands alone during the time when nature wants them most. In another place he describes the thymus as being "an appendage to the lymphatic glands, for the more perfectly and expeditiously forming the central particles of the blood in the fœtus, and in the early part of life."

In regard to the spleen, the original views of Hewson on the structure and functions of that organ have been held by many up to our own time, and are worthy still of the most thoughtful consideration. To use his own explanation, he conceived that "the spleen secretes the vesicular portion of the red particles of the blood." He was of opinion that the spleen was not really a ductless organ or gland; but that the lymphatic vessels arising from its cells are its excretory ducts, and convey the red particles of the blood into the thoracic duct, from whence they pass into the blood vessels, and are conveyed to the different parts of the body to answer the purposes of nutrition and vivification. The thymus gland in the earlier periods of life, and the lymphatic glands in all periods of life, produce lymph particles

or globules, which pass through the thoracic duct into the blood, which globules or particles he distinguished in the blood, constituting what we in our time call white corpuscles, "floating in the blood." "But nature would not make so infinitely many particles to answer no purpose! What, then," he asks, "becomes of those particles after they have mixed with the circulating blood: are they immediately destroyed? No. They are carried with the blood to the spleen; not that the spleen has any elective attraction over them, but that, being equally and uniformly diffused through the general mass of blood, a due or necessary proportion of them is received by the spleen with its arterial blood, and that the spleen has the power of separating them from the other parts of the blood, and of depositing them in the cells of that gland, where the arteries, which are spread out in the form of network upon the sides of the cells, secrete from the blood the vesicular portion, which, being thus perfectly made, is absorbed by the lymphatic vessels, which originate from the cells, and is conveyed from thence into the thoracic duct, and so into the blood vessels." In support of this hypothesis he adduced as arguments: (*a*) That the blood from the splenic vein will not coagulate, because, as he inferred, its coagulable lymph is employed by nature in the formation of the red vesicle; (*b*) That the blood of the splenic vein contains no central particles; (*c*) That in every animal that has red blood a spleen is found, but in those animals which have not red blood a spleen is wanting; (*d*) That vast quantities of the red particles of the blood are brought from the spleen by the lymphatic vessels which originate in its substance, for which reason he calls these lymphatic vessels "the excretory ducts of the spleen."

One difficulty stood in the way of the acceptance of the above hypothesis, namely, that after the spleen has been removed from the body of a living animal, red blood corpuscles continue to be made. The fact he accepted, and, indeed, proved from his own research. He explained the difficulty in a simple, if not conclusive, manner. He said, if we allow that the spleen makes the red part of the blood, we can still readily account for the reason why the spleen may be cut out from an animal, and yet the animal survive and suffer but little inconvenience; because, although the office of the spleen is to form the red particles of the blood, "*yet it is not the only organ in the body capable of doing that office;*" for he already had proved that the lymphatic vessels can, and do, produce the vesicular portion of the blood. Nature, therefore, he contended, had given the spleen as an "auxiliary to the lymphatic system, in order to the more commodiously, expeditiously, and completely forming the red part of the blood." Supposing then that the spleen be cut out, or its office obstructed by disease, nature has a resource, in exciting the lymphatic glands to form a larger quantity of red particles than they had ordinarily been accustomed to do; not a new office, but an old one carried into a greater activity; a balance of activities common to other organs and functions under the pressure of necessity.

On the Properties of the Blood.

We have seen in these last sentences that Hewson had an original way of accounting for the formation of the red corpuscles of the blood; we have also seen that he was acquainted with, and probably was the discoverer of, the white corpuscles. But he made other and original observations on the blood, and must, indeed, be admitted to have

been one of the ablest discoverers of many facts which we accept as commonplace, but which before his time were quite unknown.

In relation to the corpuscles of the blood, as they are met with in the blood itself, he discovered much. He worked well with the microscope, preferring to use the single rather than the compound lenses, made by some of the more skilful workmen of London. The single lenses of the greatest magnifying power were those of one-fiftieth of an inch focus, which, allowing eight inches to be the focal distance of the naked eye, magnified the diameter of the object only about four hundred times. In some instances, however, he used the globules of glass which the ingenious Father de la Torré presented to the Royal Society, one of which magnified the object six hundred and forty, the other twelve hundred and eighty times. On the whole, he preferred the single lenses of lower power, the globules being full of clouds, made by the smoke of the lamp used in preparing them, so that the objects could only be seen through the transparent parts of the globules, which made it difficult to get a satisfactory view of the object, and gave trouble in adapting the object to the focus of the glass.

Equipped with his single lenses of the best make, and with occasional aid from the De la Torré glass globules, Hewson worked at the red particles of the blood, and soon made the observation that by taking the particles from fresh blood, and diluting them, not with water but with serum, he could view them distinct from each other; a convenience which led him to see that they were not globules, as had been supposed from the time of Leuwenhoeck, not spherical bodies, but "flat as a guinea." He likewise observed that they had a dark spot in the middle, which Father de la Torré took for a

hole, but which, on careful examination, proved not to be a perforation, but, as he (Hewson) imagined, a central body. Next he made experiments by mixing the particles with a variety of fluids, and by examining them from different animals, from which he learned that their size is different in different animals; but, that, as Leuwenhoeck explained, their size does not increase with the size of the animal, but depends on some other circumstance than a difference in the size of the animal from which they are derived.

To Hewson we are indebted for the mode of separating the red corpuscles by adding to fresh blood a saline solution, like Glauber's salts. We are also indebted to him for another method, which I have often followed, and which consists in churning up the crassamentum into the serum of blood that has been allowed to coagulate, and then, after pouring off the mixture of serum and corpuscles, allowing the corpuscles to subside. It was he who observed first that the corpuscles of freshly-drawn blood are given to run together in rolls, like piles of money; that they are rendered spherical on the addition of water and of alkaline solutions; that they are separated by addition of some salts in certain proportions; that the normal salts of the blood effect their separation; that they are dissolved by weak acid solutions; that they are corrugated by solutions of alcohol and by solutions of some of the metallic salts, like sulphate of copper; and that, made spherical by water, they may be restored to their usual shape on addition of a neutral salt.

Most curious and important are experiments of another kind which Hewson made in reference to the coagulation of blood, that remarkable vital phenomenon on which up to this day dispute is so rife, and on which so much difference of opinion remains. I remember, when working

at the coagulation of the blood for the Astley Cooper prize essay, in the years 1853-4, how much I was struck by Hewson's work, and how often he came to mind when, like him, I was at the markets where animals are killed, taking notes of the conditions under which the blood of the animals slaughtered there undergoes solidification. Hewson was first to disprove the error that heat prevents coagulation and that cold favours it. He properly reversed this conclusion. He demonstrated that heat hastens the process, and that cold not only retards it, but, properly applied, suspends it altogether; a fact which supported a Lister of Hewson's day in the discovery that the blood of hybernating animals remains fluid during the cold months in which they live their drowsy lives.

He discovered, also, that some neutral salts, like Glauber's salts, on being added to freshly-drawn blood, prevent coagulation; but if water be afterwards added, the coagulation will follow. He found that blood tied up in a vein would remain for a long time fluid; and he held that in the vessels of the body the coagulable part is distributed, not in the form of fibrous particles, as had been supposed, but as a true fluid, specific in its nature and distinct from the serum with which it was uniformly commingled in the blood whilst circulating through the vessels.

The active cause of coagulation he did not reveal: he seems to have stopped at the effects of exposure of coagulable blood to the air. This, it is true, did not satisfy him, but he got no further. Accustomed to bleed very frequently for diseases affecting mankind, according to the practice of his day, he came to the conclusion that the inflammatory state produced a condition of thinness of the blood which prolonged the period of coagulation after the blood was drawn, and that the blood corpuscles

having time to subside, the colourless or buffy coat was left as a coat or crust on the surface of the serum and corpuscles. So, he observed, after Sydenham, that if inflammatory blood be moved by the finger whilst it is coagulating, it does not form the buffy coat, because the red colouring substance remains admixed with the whole as a red clot.

WORK AS A PRACTITIONER.

It has been customary to think of Hewson as an anatomist and physiologist merely: he was both in a remarkable degree; but he was likewise a practitioner of keen observing power and resource. Young as he was, he had gained, some time before his death, the confidence of many patients, and had acquired a good and extensive practice. His references to what occurred in the treatment of patients is the best evidence on this point. It was often his duty to draw blood, and as often, as it would seem, it was his self-ordained duty to gather information on what he then observed, and to apply the information obtained to practical service. To take one illustration, which answers for several more; he observed that when blood is being lost, either by the lancet or from accidental hæmorrhage, the tendency for coagulation quickens as the loss of blood is continued, until at last the blood clots at the wound itself, and renders the persistent flow difficult. The observation is as truthful as it is correctly expressed, and the practical inference deduced from it is as sagacious as it is practical. He remembered a remark by Dr. Hunter, "that the faintness which comes on after hæmorrhages, instead of alarming the bystanders and making them support the patient by stimulating medicines, like spirits of hartshorn and cordials, should be looked upon as

salutary; as it seems to be the method nature takes to give the blood time to coagulate;" and thereupon he goes to the markets and observes for himself that, in the fatal hæmorrhage produced in killing of sheep, the blood which comes first from the wound begins to coagulate in about two minutes, but later on, as the animal becomes weaker, it coagulates in less and less time, till at last, when the animal becomes very weak, the blood, though quite fluid as it flows from the vessels, has hardly been received into the cup before it is congealed. The coagulation, he observed also, of the blood last drawn is at once all through the mass, not step by step; from which he, with Dr. Hunter, drew a conclusion in regard to the stopping of hæmorrhage, "not to rouse the patient by stimulating medicines, nor by motion; but to let the languor or faintness continue, since it is so favourable. For in proportion as these effects are produced, the divided arteries become more capable of contracting, and the blood more readily coagulates, and closes the bleeding orifices.

The brief life-work of William Hewson commenced well, went on well, ended well. The breath of enthusiasm which wafted it along, only wafted it, never carried it into the clouds. The language in which he revealed himself is pure and simple; earnest, without being vehement; confident, without being boastful. Opinions so novel as he ventured to advance must, he felt sure, meet with opposition, and ought not, indeed, to be admitted till they had stood the test of the most careful and accurate examination. It might, therefore, be some time before they were universally allowed; but, when the facts were viewed with candour, and experiments relating to them were prosecuted with industry, they would probably direct the way to discover many operations of

the animal economy that were then considered among the inexplicable arcana of nature. In this spirit he submitted all he had done to the judgment of the learned reader. Twice he had a brush with Alexander Monro; once on the subject of the operation of paracentesis thoracis, once on discoveries relating to the lymphatic system; but these contentions only add piquancy to his arguments.

THE CLOSE OF A BRILLIANT CAREER.

With touching simplicity Mrs. Hewson records the death of our brilliant scholar. On May 1st, 1774, Hewson, in the thirty-fifth year of his age, died, as we have seen, of what we should now call septic fever, the result of a dissection wound. He had been married a little over three years and nine months, and had two sons born to him. "A better son and husband," says Mrs. Hewson, "or a fonder father than Mr. Hewson, never existed. His manners were gentle and engaging; his ambition was free from ostentation, his prudence was without meanness, and he was more covetous of fame than fortune." She describes him personally as slender in form, above the middle stature; "with a good air and a pleasing countenance, expressive of the gentleness and sagacity of his mind." Gulliver was fortunate enough to find our common friend, the late Professor John Quekett, in possession of a mezzotint of Hewson, which he published in his volume for the Sydenham Society. "It was probably," he says, "the same portrait as that spoken of by Dr. Franklin, as follows, in a letter which he wrote to Mrs. Hewson from Passy, in June 1782: 'I forget whether I ever acknowledged the receipt of the prints of Mr. Hewson. I have one of them framed in my study. I think it very like.'" The autotype forming the frontispiece of this ASCLEPIAD is from Gulliver's engraving; the autograph is from a volume in the Royal Society.

The family of William Hewson still remains. His father died at Hexham in 1767; his mother and one sister survived him. His wife after his death continued to enjoy the esteemed friendship of Dr. Benjamin Franklin, and after the death of her mother in 1783 she went to Passy and stayed the winter with him. Later, on his return to America, she received a wish from Franklin that she should emigrate to Philadelphia with her children and be his neighbour. She responded, and resided in Philadelphia until 1792, when she removed to Bristol in Pennsylvania, where her eldest son William had established himself in medical practice, and with him she resided until her death on October 14th, 1795. Her son William survived her only a few years; he died at Vera Cruz in 1802. Her daughter became Mrs. David Caldwell. Her second son, Dr. Thomas Tickell Hewson, rose to be a distinguished practitioner in Philadelphia, and was President of the College of Physicians there from April 1835 to the date of his death in February 1848. The eighth child of this descendant was the late well-known and distinguished Dr. Addinell Hewson, of Philadelphia, who was born on November 22nd, 1828, and died on September 11th, 1889, leaving behind him, to sustain the honoured name, five children, one of whom, Dr. Addinell Hewson, has succeeded his father as a practitioner of physic in the city of brotherly love.

William Hewson Secundus is alive in Texas; and from our William Hewson, F.R.S., through each succeeding generation, one member of each generation has given to medical science either a teacher of anatomy or of surgery down to the present time; namely, T. T. Hewson (1822), anatomy; Addinell Hewson (1835), surgery; and Addinell Hewson Secundus (1879), Demonstrator of Anatomy in Jeaffreson Medical College. It is also worthy of notice

that through each one of William Hewson's children, William, Thomas Tickell, and Elizabeth (Mrs. David Caldwell), there is at present a representative of medicine: Dr. David Caldwell Hewson (son of William Secundus), in Texas; Dr. Addinell Secundus (grandson of Thomas Tickell Hewson), in Philadelphia; and Dr. Thomas Hewson Bradford (grandson of Mrs. David Caldwell), in Philadelphia. Truly Hewson's descendants have fulfilled his wife's hopes; they have proved "worthy of the stock from which they grew, and do honour to the name of Hewson."*

In the whole range of medical scientific life there is not, to my knowledge, a single instance of one who in so brief a life as that of William Hewson did so much medical work so well, or made in so short a time such excellent history. He lived for scientific medicine; he died in her service; and although over his grave in St. Martin-in-the-Fields, where he was buried, possibly not far from his friend and fellow-worker John Hunter, no tomb was raised, nor epitaph written, his name remains a precious name; and his works a monument, which bear on their every page the impress of genius in the bond of truth.

* I shall have for a postscript in next ASCLEPIAD some further notes that have been promised me respecting William Hewson, together with an original letter of his, in the possession of the present Dr. Addinell Hewson, to whom I am already much indebted for useful information.

INFLUENZA AS AN ORGANIC NERVOUS PARESIS.

THE epidemic of influenza which has visited us during the past few weeks is memorable, not only from the severity and extensiveness of its course, but from the perplexing phenomena it has presented, and the diseases of an independent character it seems to have left in its train. Of influenza simply thousands have suffered, but probably not one has died; of the diseases incident to it vast numbers have died. No one dies of a common cold, severe as it may be, but multitudes die of disease induced by common cold. I look back over my old note-books, and turning to fatal records of a large number of diseases of the so-styled inflammatory type, I find, ever as the preface, that the patient was seized with cold, or took a chill, another mode of expressing the same event. Place influenza in the position of a widespread or universal catarrh,—severe common cold,—and it is in its natural position, with all that follows it; but why it should be universal is as difficult to determine, and not a whit more difficult, as is the taking of a severe cold, at any other time, by a single individual. At first it would look as if the primary affection or catarrh had spread by contagion or infection, but on fair enquiry a doubt remains.

There is often a suspicion that ordinary colds are contagious, and it is the constantly-observed fact that when a cold is started in a household it goes through the house. "Don't come near me; I have a cold," becomes a common form of expression, under the idea that there is danger in proximity. Is there danger, or is there not? I, for one, really do not know. Sometimes I have thought that colds in a house spread by a kind of sympathy; against which, as against ordinary contagion, there is this argument, that all affected may be, at the time, under one and the same influence. So it stands now in respect to influenza. I am called one day to a house to find several persons suffering with this disease, and the history supplied is that one of the sufferers having contracted the affection many miles away, where it was prevailing, brought it home with him. I go to another house to find a large establishment with every member of it free from the affection, except one member, who has never been exposed, who, in fact, has never left the house, and who alone is suffering severely. Where lies the truth? What is coincidence and what is cause in relation to the phenomenon?

I have not seen one example in which clothes or letters passed from one person to another has been assignable as a cause of catarrh or influenza. I have not seen an instance in which the influenza has spread epidemically amongst children, as scarlet fever and other contagious diseases spread. From these facts it seems clear that the diffusion is by different means than those which promote the true contagious diseases. Moreover, one attack does not prevent another.

What I have observed, however, as certain, is that the depressed members of the community—depressed by

age, by work, by nervous excitability, by late hours, by anxiety, by strain mental or physical, by confinement in close rooms, by broken rest—are those most easily affected. *À priori*, it would be assumed that men exposed to the extremest cold in the open air would be those most influenced. Railway engine drivers would, one might suppose, be specially attacked. This is not the case, and as their comparative exemption is the same, relatively, in respect to common colds, there is not much reason for wonder. It might be assumed, again, that men physically very strong would escape if any would, and that the physically feeble would of all others succumb. That is not the fact. True, the exhausted are most susceptible, but it is the nervously exhausted, amongst whom are often men of considerable physical powers, who are most stricken; whilst many who are physically weak, but nervously strong, escape.

Theory of Causation.

Those who try to trace everything in disease to germs and their effects have taken it for granted that the present epidemic is due to a microbe. They have not afforded the slightest proof in favour of their contention. The direction in which we are led by such evidence as admits of being gathered, is that the affection is nervous in its character, and depends on an influence which directly affects the organic nervous function. *It* is, an *organic nervous paresis*. The nearest approach I have witnessed to the phenomena of influenza—phenomena induced by a known cause—was in some experimental researches with ozone. I produced in my own person, by the inhalation of air admixed with ozonized air, every one of the primary

symptoms which at first demonstrate influenza. Irritability in the nasal cavities, succeeded by free secretion of watery fluid in those cavities; tightness of the chest; a distressing headache, with pain in the eyeballs; a sense of nausea. These were the symptoms induced at a temperature of 60° Fahr., and were followed by intense depression, and exhaustion. I described these symptoms as those of severe cold, nasal catarrh, passing even into bronchial catarrh. On the lower animals exposed still more determinately to ozone the phenomena were easily rendered fatally severe. Rats and guinea-pigs submitted to its influence died from it, although the air was kept in constant current, and carbonic acid was removed as fast as it was formed. The mode of death was invariably the same: it was from congestion of the lungs, or, as it would be called in the human subject, congestive pneumonia. With this there were bronchial symptoms, and if the fatal event was not too rapid there was hydrops-bronchialis. There was also some congestion of the kidneys, and of other vascular organs, although not in so marked a degree as in the lungs. All round, the symptoms induced by ozone constituted a perfect synthesis of influenza, followed by pneumo-paresis, as we have seen so often in the current epidemic. In the researches on ozone it was observed that the phenomena induced were modified by temperature. Effects from the same ozonized atmosphere, that were rapidly fatal at 70° Fahr., were extended over much greater lengths of time, at 40° to 45° Fahr.; but the general and special results were the same in the end. Moisture also made a considerable difference, acting after the manner of cold, and prolonging the series of changes. I observed another fact that ought not to be forgotten, namely,

that the presence of water vapour in the ozonized air to the extent of rendering me unconscious of inhaling a foreign body, did not prevent the development of the distressing symptoms, and as it seemed to me intensified the headache.

The physiological action displayed was on the organic nervous system throughout, and consisted of a paresis or reduction of controlling power of that system. This was evidenced by the vascular congestion, the overflow of mucous secretion, and the sense of feebleness upon the subsidence of the first acute symptoms.

Until by future research it be found that the atmosphere during epidemic catarrh is changed in character, either by having passed over some surface which has modified its physical constitution, or has charged it with infinitely-minute particles leading to an equivalent change, it would be improper to affirm that these catarrhal epidemics are due to an allotropic change of the oxygen. But this may safely and strongly be said, that from synthetic observation the evidence in favour of such a view is more striking than any other line of evidence that has been adduced; and that further research is demanded in relation to atmospheric states during the existence of these great and distressing outbreaks.

Means of Treatment.

The treatment of influenza by medicinal means has been, up to the present time, an utter confusion. I have met with two or three of my brethren who were firm in believing that the expectant treatment, in conjunction with careful regimen and hygiene, was by far the best and soundest. Amongst those who entertained a desire for some kind of medicament there

was no such unanimity. One was for quinine from the first to the last; another thought well of antipyrin; a third had "cured" all his cases with salicylate of soda, under the idea, not altogether a bad one, that there was a rheumatic element in the epidemic affection; a fourth was inclined to the use of salines, especially chlorate of potash; a fifth considered aconite, "in guarded doses," the *beau idéal* measure, especially when the fever was very high. These were individual expressions at large, what the lawyers call "flying opinions," sure tokens that there is no principle at the bottom; sure tokens of a wandering knowledge which, amongst these same practitioners themselves, would be considered woolgathering if it were applied to the consideration of the best mode for rendering a person insensible to pain for a surgical operation, or for any other measure based on a correct estimation of cause in relation to effect. This confusion will remain until we arrive at the cause; after which we may possibly alight upon a common remedy. In my experiments I found the speediest and most direct relief from breathing the vapour of ammonia. That vapour directly negatived the irritant effect of the ozone, and removed the headache; for which reason I have since always resorted to it, and with great success, in meeting the first symptoms of catarrh. But when the nervous disturbance has become established, this local measure is ineffective. Wanted, a method for restoring the normal tension of the organic nervous system. That is all, and that is cure. One word more, in conclusion. The successful remedy when found will not be an alcoholic stimulant. According to my observation, alcohol has added largely to the dangers of influenza.

"THE DOCTOR" AT THE ROYAL ACADEMY.

THE art critics vie with each other in praise of Mr. Luke Fildes' picture, "The Doctor," at the Royal Academy Exhibition of this year. There need be no wonder at the admiration, for, as a work of art, nothing better of its kind could be. The canvas lives and literally glows. We, who are growing advanced in medical life, have seen the picture, in its stern sad reality, times and often. Every point of the picture is of itself a picture. The sick and perhaps dying child is nature herself. In health the child might, under happy circumstances, be a child of beauty, but sickness has so far progressed, that the finger of threatening death has rubbed out the tinctures of life. After actual death, one face is so closely like to other faces of similar type, it is difficult to tell one dead face from another, or prove thereby identification. In the present instance things have not gone so far, and the artist probably leaves us in hope that the animal fire may yet revive; but the dissolution is impending, and the child might, therefore, be any child not positively ugly, but either commonplace or beautiful, when in the bloom of good health. As she lies, for it is a child of the gentler sex, she is not in pain, and she sleeps flexed. She is not Hippocratic, neither is she much wasted. Some acute

disease has caught her; and, rendered comatose, she seems to be sleeping into the final sleep. She does not suffer, and her surroundings are not such as to suggest that the brightness of a promised life of happiness is before her should she recover; her improvised bed is a bed of poverty, over which the light from under the shade of a paraffine lamp of the ordinary cheap sort throws a gleam of sadness; a sunless stain telling of future struggle if this first struggle should be fought through.

The eye of the observer is drawn from the sick-bed and its occupant to two figures on the right hand, the father and mother of the child. They are near a window, into which the first rays of the rising sun are diffusing the dawn. Another point, half scientifically true, half sentimentally true, half poetically true, is made here. The coming of day is always, ideally at least, a critical hour in relation to death. As a matter of fact, death often takes place at break of day; yet not so often as to give any established rule, making the hour more fatal than other hours. But at this period the watchers are bent low; their spirit is low; their heart is wearied; anxious friends feel the depressing influence; nurses who have been at their duties all the night feel it; the most experienced and inured practitioner of medicine feels it: so about this time there is a concentration of hopelessness and gloom which feeds a weird superstition in which the strong as well as the weak participate during the passing scene. These parents before us feel acutely this moment of despair. The sun god mocks them! Why should he rise again to bedeck the world outside in renewed splendour, and reveal to them a world in which the little sun of life which has been theirs shines no more? The mother

leaning over a table buries her head in her arms so that her face is hidden; but her posture shows her agony. We almost hear her sobbing throes, almost see her tearful eyes, almost feel her palpitating heart and divine her deep despair. The husband, of sterner stuff, stands mute in his pain. One of his hands rests firmly on the shoulder of his wife, as telling her he is near, to sustain her; to indicate that his strong arm is still with her, to work for her and protect her, perhaps to suggest also that all is not over; nay, may be, there is yet hope. For the rest he is more resigned than she, yet little less heart-broken; his face, visible to us, scarcely bears to be looked at; it is expressionless as the child's, but is awake; a deathly face awake and waiting in expectation a message of death at any moment.

The central figure of the picture, the figure that makes and fills up the body of the picture, is "The Doctor." The name is happy, and by general acceptance and popular voice is correct; and yet, according to the strict meaning of the title, it is incorrect, for everything is done to show, not a doctor, in the original sense of a learned man, but an earnest, sympathetic, and thoughtful attendant on the sick. Every semblance of learning is put aside. There is no book, no philosophical instrument, no garment of learning. A common teacup and a bottle are all the instruments of aid that are in sight. The Doctor himself is middle-aged, a strong, well-built, and a handsome man. He sits by the side of the sick-couch, his eyes turned earnestly on his little patient, as if he were counting up the chances for life or for death, and as if the balance were as fine as it could be. He is a man too far in the valley of experience to be misled by enthusiasm, or to be led on by faith in what his skill can do; whilst, at

the same time, he has seen so many strange recoveries when he least of all expected them, he is not as one without hope. If a layman were to ask him what is going to happen, he might reply, "Well, there is youth on our side; and, prepared for the worst, we must act for the best;" but if the layman were clever enough to get at his actual mind, he would find him saying to himself, with the Danish Prince, "Why, what an arrant knave and fool am I," to sit here as a healer, powerless as the rest: or, thinking of other cases he has seen of the same nature, he may be trying to remember if any one plan of treatment has really been better than another. Evidently he hesitates, not as if he had done something and was waiting for the result, but as if he had not seen anything that could reasonably be done, and were waiting the action of that capricious jade, Nature, who, caring nothing for the woman's tears, nor the man's distraction, is pursuing her own relentless course.

As a work of art "The Doctor" is a masterly picture; as a work of history it is not less masterly. To the medical historian of a future age it will be a treasure, feeding the eye with truth of delineation, which our diffuse literature will do nothing to assist. There is here, in concentrate history, a page revealing, at a glance, the current genius of physic in all its strength and all its weakness. Theories of medicine abound by the score; declarations of practical results from diverse practices stretch out by the league; letter type histories of remedies lie pile upon pile, until it is necessary in sheer despair to give them up. But this picture is a grand total; a child, who ought not to be so much as ill, lying at the gate of death; her parents prostrate with grief; Nature calling up the

sun to show how little it signifies to her whether the house shall be one of gladness or mourning, hope or despair; and a healer with his hands at rest looking on and waiting the result, content to feel that if he has done little good he has at least avoided the errors of his forefathers, and has done no harm. A brother physician standing by me, and aware that the distinguished artist was known to me, remarked pensively, "Your friend might be a 'Doctor' himself, he presents the spirit of the part so faithfully." Yes, it is so; not because my friend "might be a Doctor," but because, in our present environment, there is an universal sympathy through which science and art become embodied. The picture belongs to the age of the artist; could have been painted in no other age. On the subject with which it deals it exhibits the time, its form and pressure. Whether the form and pressure is good and healthy is our business, not his. On one hand it is not good, not healthy, because it suggests too faithfully a painful scene, which our art ought to be able to laugh to scorn, as too monstrous to be possible. On the other hand, by showing us how weak we are, the scene recalls us to the sense of what we ought to be. This doctor is a type of healer waiting to be evolved into something far grander and more powerful in his vocation. His sympathies are strong and generous, his heart is in the right place; but he has not yet learned enough, albeit he is dubbed doctor. He cannot, of a certainty, save that child; he cannot put that little lamp of life, still burning, under such a condition that it cannot go out, but must light up again; he cannot give that mother and father assurance of his power; and if even the little life should come round, and they should assign to him the credit of being the saving cause, he

dare not, if he be true to himself, lay the flattering unction to his soul that their sincere gratitude is honestly deserved. These reflections, however, only make the debt we owe to the artist the more profound. He marks for us, as it were, a new starting-point, from which we ought to set forth the more vigorously towards accession of power, with more certainty, more practical assurance of safe intervention, and more of the quality of positive of skill, based on a scientific method, as exact as it should be triumphant.

Such is the effect of Mr. Fildes' picture on a mind not content to sit by, merely, and watch, like the hero of his canvas. I feel ashamed that those parents in my presence should mourn, and that young life die. The picture calls out to me, "Go back to your studies, unlearned man; work still closer and more earnestly while you can work at all, to redeem your misspent time; and though you may not accomplish the victory yourself, work the more to help on the day when some who shall follow you shall give a future academician the opportunity of painting a doctor who is a 'Doctor' in deed as well as in name."

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."

"ELECTRICITY IN MEDICINE AND SURGERY."

[BY G. A. LIEBIG, PH.D., AND GEORGE H. ROHE, M.D.]

IT has been the endeavour of the authors to set forth concisely in the pages of their work the fundamental principles involved in the application of electricity to medical and surgical practice.

The work (F. A. Davis), which is throughout profusely illustrated, is divided into three parts. The first part is devoted to a description of the various forms of electrical and magnetic apparatus likely to be of service in medical practice; the second part relates to the effects of electrical currents upon the various tissues and organs of the body in health, how these effects are modified in diseases, and the methods by which the modifications may be utilized for purposes of diagnosis; the third part treats on the applications of electricity in the treatment of disease. The first part of the volume is the best. In this part the authors are on safer descriptive ground than in the succeeding two parts. They deal with proven facts and with recognised physical laws. Here and there,

from limitation of space and desire to condense, they pass out of the range of the general medical reader who has not made the study of the history of electricity a part of his research; that is to say, they take for granted knowledge of a technical kind which to them is easy enough, but which is not always easy to those who have even a fair elementary grasp of electric and magnetic phenomena. They are also a little irregular in their facility of exposition. This is manifested in the pages on magnetism, when carefully compared with those on electricity. The theories of the nature of magnetism are more clearly stated: a difference which may be necessary, as belonging to the different topics, one of which—magnetism—is always more understandable than the other.

The second part of the book has on every page something of service to the practitioner, and the illustrations, indicating in the most striking manner the action of electric vibrations on muscular action, are most instructive. Chapter II. in this part, on electro-diagnosis, is also instructive, but it is strange to me, as well as new, that the authors should treat so cavalierly Professor Hughes' sonometer for diagnosis of deafness. They say of this instrument that it is of "limited applicability." Well, I was the first to use this instrument after my good and distinguished friend, its inventor, put it into my hands in the year 1879 to test and bring before the Royal Society. I have had it daily in use since that date, and of its kind, as an electrical instrument for diagnosis, there is not one that I have found so useful or so largely applicable. It is accurate in what it explains to, it may be, a fault, as some would think; for it tests all treatment with a degree of nicety which is not always pleasant; and to doubt its

applicability, in the widest and truest sense, is simply not to know its capabilities. I can only suppose that the instrument has not been employed by the authors to the same extent as other instruments, and that they have reported too much on hearsay; an error I trust they will correct in a future edition of their treatise, which is certain not to be long delayed.

The last part of the volume, dealing with the applications of electricity in the treatment of diseases, is the least satisfactory, as might be expected. At the same time, it opens up a wide field of service for electricity, as an agent to stimulate nutrition; to excite muscular contraction; to modify the blood supply to a part; to relieve pain; to exalt sensation; to destroy tissue by the slow process of electrolysis; to remove a part by the rapidly-acting galvano cautery; and, finally, to furnish light and a medium of sound-transmission for diagnostic purposes. This is what the American commercial world call "a large order;" but it must be admitted that, within the modesty which our authors strictly keep, it is a correct order, except when sometimes, perhaps, a little undue enthusiasm eggs them on to enforce practices rather beyond what are always easy to do and certain to score. But judgment on these points may be left with readers, for whom it is a sufficient duty, in this place, to set out the mental board, leaving them to take the proofs from their own investigations. They certainly will not starve for want of useful occupation, for many years, if they will make a careful study of Drs. Liebig and Rohe, on "Practical Electricity in Medicine and Surgery."

"CLINICAL INSTRUCTION IN ASYLUMS FOR THE INSANE."

[BY DR. RICHARD GREENE.]

ON the 25th of February last Dr. Richard Greene, Superintendent of the Berrywood Asylum, Northampton, read a paper before the Hospitals Association, at Westminster Hospital, on the above subject, a paper he has since published in a separate form. For some time past there has been a movement on foot for the establishment of one or more hospitals in which a systematic attempt should be made to cure insanity absolutely, and in which there shall be the most perfect form of clinical instruction on insanity, its causes and its cure. Dr. Greene looks on this projected scheme as a work of supererogation. He contends that there is no simple system of cure of insanity, by special treatment, in the same sense as in the cure of a wound. Insanity is a law unto itself, and the community afflicted with insanity requires, not merely treatment of a curative tendency, but a constant supervision that passes beyond what is considered too often as cure, but is really, in many instances, nothing more than a temporary return to mental equilibrium. At the same time, he does not despise attempts at cure, neither does he oppose clinical teaching; in fact, he commends and recommends both. His argument is that there is no need of a separate institution for this set and particular purpose. The institutions already existing offer, he believes, every facility for both these intentions, if the advantages supplied were properly administered. He is in favour of a modification of management of asylums by which, in certain cases of promising kind for cure, there should be a true hospital system of cure within

the asylum, with all the appliances of medical and surgical art; and he pronounces, in the strongest terms, the promotion of sound clinical instruction to all practitioners of medicine. Further, he advocates a better system of education amongst the people at large, by which they may be trained to avoid the reproduction of insanity as an hereditary disease. But for the grand advances thus prescribed he sees no need for separating the insane into classes fitted for special hospitals, to be supported by special public funds and conducted by men specially trained, not in the study of insanity as a whole, but in some particular forms of it.

When the idea of starting a central hospital in London for the cure of insanity was first broached, it sounded so plausible that many were smitten with it. It is fortunate, therefore, to find the scheme subjected to analysis, and certain points of it in which it is weak and impracticable criticised by one who is not only an accomplished physician, but whose sympathies and leanings are towards general, as distinct from mere professional, privileges. That Dr. Greene carried, perhaps, every one of his audience—including the distinguished chairman, Dr. Lockhart Robertson—with him is the best evidence of the force of his argument.

"THE DWELLING-HOUSE IN RELATION TO TUBERCULOUS CONSUMPTION."

[BY R. THORNE THORNE, M.B., F.R.S.]

DR. THORNE THORNE, with his usual directness of purpose and comprehensiveness of knowledge, gives the following salient points on the dwelling-house in relation to tuberculous consumption :—

Conditions of Dwelling-House tending to the Promotion of Tuberculous Consumption.	*Conditions of Dwelling-House tending to the Prevention of Tuberculous Consumption.*
1. A soil either (*a*) naturally damp and cold; or (*b*) subject to the influence of the rise and fall of a subsoil water lying within a few feet of the surface.	1. A soil which is dry (*a*) naturally; or (*b*) freed by artificial means from the injurious influence of dampness, and of the oscillations of the underlying subsoil water.
2. A dwelling-house of which either the foundations, the area they enclose, or the walls, are, by reason of faulty construction or otherwise, liable to dampness.	2. A dwelling-house so constructed as to be protected against dampness of site, foundations, and walls.
3. Such immediate surroundings of the dwelling-house as tend to prevent the free movement of air about it, and its ample exposure to the influence of sunlight.	3. Such open space on at least two opposite sides of the dwelling-house as shall secure ample movement of air about it, together with its free exposure to the influence of sunlight.
4. Such structural defects as would prevent the maintenance within all parts of the dwelling-house of ample movement of air by day and by night, and free exposure of its habitable rooms to daylight.	4. Such construction of the dwelling-house as will secure for its habitable rooms, and throughout its interior, free movement of air by day and by night, and the free access of daylight.

OPERÆ PRETIUM EST.

UNDER the title "REPORT *on the State of the Blood and the Blood-vessels in Inflammation, and on Other Points relating to the Circulation in the Extreme Vessels; together with a Report on Lymphatic Hearts and on the Propulsion of Lymph from them, hrough a Proper Duct, into their Respective Veins*," Mr. T. Wharton Jones, F.R.S., publishes (through Messrs. Baillière, Tindall, & Cox) a small volume of sixty-seven pages, in which he condenses the richer part of his important labours in physiological science. The reason assigned by the author for the publication is that the pages are addressed to authorities "who may consider the faithful instruction of students of medicine, in a real knowledge of the phenomena of the circulation of the blood, to be of

paramount importance." Thence he proceeds, chapter by chapter, to re-unfold his many original and able observations on the blood, for modern thought and learning. The work is that of a man of genius of the highest order, whom every one ought to understand, and who has never been sufficiently understood.

"DIGESTION AND DIET" is the title of a series of collected contributions by Sir William Roberts, M.D., F.R.S., published by Smith, Elder, & Co. They consist of reprints of the author's now well-known Lumleian Lectures, and of two lectures on Dietetics and Dyspepsia, given at Owen's College in 1885. The volume is made up of four sections: I. Digestion and the Digestive Ferments; II. Dietetics; III. Preparation of Food for Invalids; IV. Dyspepsia. It is an excellent book of reference, practical, and often very original.

TO CORRESPONDENTS.

SPECIAL NOTICE.—*Many readers of the* ASCLEPIAD *having requested permission to have, for framing or for the album, extra copies of the autotype portraits published with the biographies, the Author has pleasure in stating that every one who sends him intimation, to* 25, *Manchester Square, of having complied with the terms of publication as a subscriber, will receive at the close of this year separate copies of all autotypes published in the volume for* 1891.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD *is exclusively by the Author whose name is on the title-page.*

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD *should be addressed to the Author at his residence,* 25, *Manchester Square, London, W.*

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, *Paternoster Square, London, E.C.*

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

May 15th, 1891.

APPENDIX OF ACKNOWLEDGMENTS.

"A." "Evolution: the Work of a Great Intelligence." By "W. A." London: Simpkin, Marshall, & Co. 1890. [A curious little work in the guise of a correspondence between two lovers, on matters which surpass love in the ordinary meaning of the term, and bear on some of the deepest problems of life.]

ANDREW. "The Harveian Oration, delivered before the Royal College of Physicians, 1890." By JAMES ANDREW, M.D. Oxon. London: Adlard & Son. 1891. [Sustains admirably the classical annual orations in honour of Harvey.]

ANGELL. "Municipal Work and Sanitary Progress." An Address delivered at the Institution of Civil Engineers, by LEWIS ANGELL. London: William Clowes & Sons. 1891.

ANON. "Ambition's Dreams in Two Fyttes." London. 1890.

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ON PEROXIDE OF HYDROGEN: A PHYSICAL-MEDICAL RESEARCH.

CONTINUED FROM A RESEARCH COMMENCED IN THE YEAR 1856.

PART III.—FURTHER PRACTICAL RESEARCH ON THE APPLICATION OF THE PEROXIDE AS A REMEDY.

Uses of the Peroxide in Disease.

In Epilepsy.

In Asclepiad, Volume II., page 346, I have related the results of some trials of the peroxide of hydrogen in the treatment of epilepsy. The reader is referred to this short essay, but I may state here in abstract that the facts relating to five cases of typical character are there related. In all of them the peroxide seemed to render useful service; and the last case mentioned in that paper deserves a special note here, because the patient still survives, far advanced in age. This patient, the wife of a professional man, was subject thirty years ago to frequent seizures of severe epileptic character. I prescribed for her two fluid drachm doses of the solution of ten-volume strength twice daily in water, and from the first administration she and her friends were sincere in the belief that the attacks were reduced both in frequency and severity. For seven years she held the

remedy always at hand, convinced that whenever she ceased to use it the attacks always returned. She continued to resort to it for some time afterwards, and finally made a perfect recovery. Of late years she has suffered from partial paralysis of the lower limbs, which has confined her greatly to the house, but now, bordering on eighty years, she is in fair health, eats and sleeps well, and enjoys a moderate share of mental activity. I should add that more than once in the course of treatment under the peroxide this patient had a slight ptyalism, which did no mischief at the time, but which she thought relieved her of symptoms of oppression and weight in the head attended with vertigo.

A systematic investigation of the value of the peroxide in epilepsy, and especially in what is called "Jacksonian epilepsy," is worth the labour. The dose of the ten-volume solution may begin at one fluid drachm, and be increased to two drachms, twice daily, in water sweetened with glycerine.

In Anemia.

In 1862 I had prescribed the peroxide in forty-four cases of anemia, and I have repeatedly prescribed it since for anemia. Of itself it has not, I think, any specific virtue in anemia, but from the first it seemed to me to increase the value of iron as a remedy. I have usually ordered it in combination with small doses of sulphate of iron, one grain of the sulphate with one to two fluid drachms of the ten-volume solution in two ounces of water, three times a day. In one example of pernicious anemia I carried the dose of the solution up to four fluid drachms, with the small dose of iron three times a day, and with very good results. I speak guardedly on the value of the solution in anemia, because before and since, in treating the disease,

I have seen so much good arise from free purgation, I have failed in no instance to add that method of treatment as a necessary eliminative measure. The peroxide is on the same lines, and seems to me to promote both biliary and pancreative secretion, on which, perhaps, its value largely depends. But it gives also effect to the iron, and increases the action of the small dose, possibly by causing absorption of it from the alimentary canal into the circulation.

In Mesenteric Disease and Struma.

The solution promised to be useful in mesenteric disease, and from the first I have prescribed it in this malady. It is not a remedy very acceptable to children, for it leaves in the mouth and throat a metallic taste, which is objectionable. I have endeavoured to get over the objection by giving it in combination with glycerine—one fluid drachm with the same quantity of glycerine in six drachms of water and with twenty minims of rectified spirit. An agreeable mixture is produced by this formula, and I have much reason to be satisfied with the effect. The service most appreciable from it is the improvement in digestion which almost invariably takes place, and which renders the administration of cod liver oil and iron more acceptable and useful. This observation extends to its employment in strumous affections generally, and in two cases of enlarged strumous glands in the neck I had a suspicion that the enlargement was reduced under the influence of the medicine by a direct effect. It is not unreasonable to assume that a medicine which excites free action of secreting glands, like the salivary, might exert an action on an enlarged and strumous gland; but I do not press this view since the obvious improvement in the general health may have accounted for the local improvement, which was unmistakable.

In Diphtheria.

Owing to the observation made on the effect of the peroxide solution in causing separation of dead structures, I have several times used it, since 1864, as a local remedy for diphtheria, in the treatment of which hitherto inscrutable disease I have had the most painful of human experiences. Applied to the throat of diphtheritic patients, the solution of the peroxide, either alone or in combination with glycerine, causes, without any doubt, a rapid separation of the false membrane, and produces a favourable condition of the local surface. But here, I regret to say, the benefit, and it is a benefit, ends. On the general or constitutional conditions it has not as yet, according to present modes of using it, been efficient. It does not touch the collapse. I name this because I see that some writers, on observing its good local effect in cases that have, for some other reason, recovered, speak of the peroxide as, nor more nor less, than a specific in diphtheria. It has as yet not won that position. At the same time I invariably prescribe it locally in diphtheria, not only to the throat, but as a wash for the nostrils when there is ichorous and offensive discharge. I have hope also of seeing a way to its more successful internal employment.

In Intestinal Affections.

The administration of the peroxide solution as an injection, *per anum*, was a line of practice that occurred to me about 1870 in a case of cancer of the rectum, accompanied with a free and very offensive discharge. The action of the solution in destroying the odour, and, combined with tannin, in limiting the discharge, was most useful. Soon after that I was summoned to a patient suffering from what I may designate as strumous caries

of the sacrum, with adhesion of the lower intestine and copious escape of pus into the rectum. A more painful case, or one more difficult to manage, I do not remember to have seen. Here I prescribed the ten-volume solution, with tannin and dilute hydrochloric acid, by injection, with the best results. The disease proved fatal, for the whole of the sacrum was involved before the use of the oxygen solution was commenced, and clear indications of paralysis of the lower extremities had also shown themselves. But the solution exerted such a controlling influence, and rendered the discharge so healthy, I could but regret that the treatment by it had not been brought into action at an earlier stage.

In a case of what was called chronic dysentery, with frequent evacuations of a fluid and offensive character, I prescribed the solution, with tannin, in ounce doses diluted with a pint of tepid water. The injection was thrown into the bowel with a long O'Byrne tube, and as oxygen gas was liberated freely in the intestine soon after the injection was thrown in, evidence was supplied of the presence of purulent fluid, or of some vitalised matter from blood. The result was very satisfactory, and led to such a rapid and sound recovery I can recommend the repetition of the same practice in similar cases with the utmost confidence.

Use by Inunction.

From a knowledge of its disintegrating power I have, for many years, used the peroxide as an inunction for the surface of the body, in patients presenting cutaneous exfoliation, as after scarlet fever and measles. In these instances ozonic ether is used instead of the aqueous solution, because of the facility with which it mixes with the oil or lard required to make an ointment. The body

is rubbed from head to foot with the ointment, the spaces between the fingers, toes, and flexures of joints being included. The application is very agreeable to the patient, and the effect in removing obnoxious odours is most marked. But the grand advantage lies in the circumstance that the process seems to remove all danger of infection from the convalescent after three or four applications.

In Eczema.

As a general rule, oily and fatty dressings do not agree well in chronic eczema. But I once gained considerable advantage in treating extensive chronic eczema, which had previously and for a long time been intractable, with this form of ointment. In other eczematous cases, with exudation of blood, I have employed the aqueous solution combined with tannin, or with tannin and a small proportion of tincture of aconite, when there has been much irritation, with equal advantage.

Uses in Diagnosis.

I have found the peroxide of hydrogen, both in its aqueous and ethereal forms, of great service in diagnosis. It diagnoses pus from mucus.

In cases where I am in doubt whether matter expectorated by the lung is or is not purulent, I place the expectorated matter in a test tube containing the solution. If the matter cause no evolution of oxygen there is no purulent matter present. If the expectoration produces liberation of oxygen the evidence of pus is strong; and if the same event occurs after all carbonic acid is removed the presence either of pus or of some plastic element of blood, or cellular tissue element, may be affirmed of a

certainty. In the large majority of instances it will be pus that is present.

The diagnosis extends to cellular tissue. Injected into a closed mucous cavity, like the bladder, the surface being in a healthy state, and the viscus having been emptied of urine and washed out with warm distilled water, there is no liberation of oxygen from a neutral solution of peroxide. But if there be any ulceration, by which a surface of cellular tissue is laid bare, or if there be pus present, then the escape of oxygen from two or three ounces of a ten-volume solution injected into the bladder will be so free that the gas may be detected as it escapes from the end of the catheter by its relighting a partly extinguished taper. The same test might be applied for the diagnosis of an ulcerated mucous surface, or presence of pus in the uterine cavity, and possibly also the stomach, but no opportunity has been offered me of testing in these last named cases. I may, however, add that no more mischief could follow from the test than would follow from the injection of common water or common atmospheric air, if the solution used be chemically pure; on the contrary, the ozonic quality of the oxygen in solution would have a good effect. It might be sound practice, indeed, to sustain a gentle continuous current of the fluid through the cavity in extensive ulceration of the bladder or uterus, accompanied with offensive discharge.

SUGGESTED NEW PRACTICES FROM PAST EXPERIENCES.

In the course of original research it often happens that a number of side suggestions and thoughts occur to the mind that were not dreamed of when the research commenced. In this way theory is brought out, which,

being an induction from experiment, may easily pass into new practice of a valuable character. As a rule, it is good for an investigator not to advance theory, even of this order, until he himself has put it to the proof; but there are exceptions to every rule, and here seems to me to be one. Some suggestions leading to important lines of practice have forced themselves on my attention, and call for proofs I can hardly hope to offer in my own lifetime, but which younger men may supply. I, therefore, proceed to state these suggestions without reserve; and this the more readily because in some instances the way towards practice has been opened for a considerable distance with perfect safety.

Suggested Use in Typhoid.

Let me take as a first illustration of this kind an extension of the practice of injecting the peroxide solution into mucous canals. I have indicated, as a practical fact, that the solution can be injected into the alimentary canal, and that in more than one example it deodorised and neutralised an offensive discharge in a most effective manner. I would venture further. I would recommend the practice in the treatment of typhoid. In typhoid it would be possible to throw into the canal, by this means, twenty pints of ozonised oxygen per day by using only one pint of the ten-volume solution. The gas when liberated would diffuse through the whole of the canal, and would decompose and oxidize the decomposing exuded products, which, by secondary absorption, are the cause of the relapsing febrile seizures, with an efficiency possessed by no other remedy. It would be like exposing the decomposing ulcerated surface to sea air.

Use for Hypodermic and Subpulmonary Injections in Asphyxia and Chloroform Collapse.

As in the physiological research I found no danger from injecting the neutral solution of the peroxide into the cellular tissue, I have ventured the same on the human subject, and I fully expect that the practice will in time be largely adopted. In asphyxia, as after drowning and after suspended animation from chloroform and other narcotics, a hundred pints of oxygen might easily be transfused, hypodermically, by using the thirty-volume solution; and, with the temperature of the solution raised to blood heat, the diffusion of the gas through the cellular tissue would enter the blood rapidly, and tend to give a new force to the circulation. I should even go one step further in such extreme cases. In sudden collapse from chloroform, if the natural respiration had actually ceased, and death seemed to have occurred, I should, to save time, inject the lung substance directly on both sides of the chest with the warm solution. The diffusion of the gas through the lung structure would arterialise the blood remaining in the lung, and the mechanical pressure exerted within the pleura against the elastic lung structure could be made, by up and down piston action, a new motor to the circulation, compressing the blood towards the left side of the heart, and producing a current of blood from the right side.

An Automatic Syringe.

For these subcutaneous and subpulmonary injections of the peroxide, I have invented a syringe which I call the "automatic syringe," and which Messrs. Krohne & Sesemann have very neatly constructed. It is a small

graduated instrument, having a feeding tube for charging it on one side, and an exit tube for discharging it on the other side. The syringe is automatic in the sense that when both tubes are in place the mere act of working its piston up and down fills it and empties it. The quantities of fluid received and delivered by it vary from ten minims to four fluid drachms, and at the end of the exit tube it can be fitted with injecting needles of different sizes or with a nozzle, according to requirement. With this syringe the peroxide solution can be injected, subcutaneously, or into deep structures. By attaching the delivery end to an enema tube it can also be used for the intestinal injection, and by attaching the same end to a catheter, it can be employed for injecting the bladder. The advantage in every case is that the current can be kept up when once it is started, either for a small or large injection, and that the admission of air is impossible. The syringe might be used without any complication for the transfusion of blood and for aspiration as well as for transfusion; but it has been specially adapted for the oxygen solution. There would be no danger of injecting four ounces of the thirty-volume solution, rendered neutral, and warmed to the normal blood temperature, into each lung; the water might produce a little œdema, but that would be rather favourable than otherwise under the circumstances, by moderating the oxidation, and the water would soon be picked up, and carried into the circulation by the capillary vessels.

Use in Cavus of the Lung, and in Early Stages of Phthisis.

Bearing also on this subject of subpulmonary injection there is another use of the solution which has often occurred to my mind, and which would be perfectly

legitimate as a practice, when the powerful action of the peroxide in destroying decomposing and purulent matter is taken into account. I suggest that in cases of cavus of the lung in phthisis, where the cavity can be clearly defined, there would be no risk of injecting the cavity, through a fine needle, with the solution, and of oxidising all the fluid the cavity might contain. Again, in dangerous hæmoptysis it would be most legitimate practice to inject into the pulmonary structure the oxygen solution saturated with tannin. The oxygen could do no harm, and the tannin, which would be quickly transformed into soluble glucose, would merely charge the blood with a useful saccharine food, with the advantage of making the lung, in a small degree, a kind of second stomach. Still more, it is quite worthy of serious consideration whether it would not be good practice to inject the solution in small quantities by the subpulmonary method in the early stages of phthisis. The oxygen would destroy tuberculous exudation more quickly than any other agent, and I can see no danger of diffusing oxygen through an organ that is made for the sole purpose of receiving and condensing oxygen. The danger of hæmorrhage could be met by combining tannin with the solution.

I name this new mode of treating the lung substance, the *subpulmonary*, but we may learn also how to transfuse the peroxide solution without risk and with benefit, into any organ of the body, not excluding the brain and nervous centres themselves.

A Danger to be Considered.

A danger that might arise from injection of the peroxide solution into the tissues would consist of delivering into the tissues so much oxygen *at once* that the blood would

not absorb the gas with sufficient rapidity to allow afterwards a free course through the minute circulation.

The report of an experiment on this point will illustrate what I mean. A two-necked flask is charged with six ounces of defibrinated blood from an ox. The blood must be deprived of all free carbonic acid by being washed with oxygen. It is of bright arterial colour, and if a current of air free of carbonic acid be driven through it into lime-water, there is no trace of carbonic acid. But when to this blood neutral peroxide solution is added, there is, even though it be already arterialised to some extent, brisk action. There is further oxidation, there is distinct elevation of temperature, and there is more escape of gaseous matter from the free tube of the flask through the lime-water. The gaseous matter escaping consists partly of liberated oxygen, but, as may be shown by the precipitate in the lime-water, mixed now with carbonic acid. Thus it is clear that liberation of carbonic acid in the blood circuit from the use of peroxide may occur; and as, within the circuit, there is no means of escape for the gas or gases, there is the same danger as there would be from blowing air into a vein. I observed this fact while watching the experiment recorded in ASCLEPIAD, XXIX., in which I restarted respiration in a lower animal, after death from chloroform, by injecting peroxide solution into the peritoneal cavity. The respiration was so good that life seemed to be returning until the vital functions were arrested from some new cause telling upon the circulation. After death it was found that the right side of the heart and large veins were filled with a bright arterialised blood richly charged with oxygen gas that had been absorbed from the large veins, and with which carbonic acid gas was also admixed. The prevention of this source of danger has to be worked out. It may

possibly be done by using a weak peroxide solution, or by using an alkaline solution. Meantime the danger ought to be known and the nature of it understood, since without being understood it might interfere materially with the proper application of the peroxide in its most useful and innocent administrations.

Use in Tetanus and Hydrophobia.

I have spoken of the effect of the peroxide in relaxing muscular contraction, and the facts I have observed in this direction lead me to suggest its use in two particular diseases of the spasmodic type, hitherto so intractable—I mean tetanus and hydrophobia. Any one who has watched the course of one at least of these diseases (tetanus) will feel that the treatment most of all hopeful, if it could be realised, is a means for preventing the muscular tetany, for producing free glandular elimination, and free oxidation of the septic blood. In the peroxide solution we have all the requisites; and in the next case of tetanus that comes under my care it is my intention to put the remedy to the completest trial. I shall inject the muscles, in groups, with the neutral ten-volume solution, heated to blood temperature, adding, if the oxygen be not alone efficient in reducing the tetanus, a weak alcoholic solution of amyl nitrite.

Mechanical Use.

One other purpose for the peroxide must be named; a purpose purely mechanical, and yet not to be slighted. In the process of being liberated from its solution the gas exerts an expansion, which in many instances may prove of great service. In this mechanical mode the solution could be used in obstruction of the bowels from intestinal involution. It could be used for causing distension of the bladder when the sound was being employed;

and perhaps in stricture for producing dilatation of the passage posteriorly to the stricture. In the same manner it could be brought into play for dilating the uterine cavity; while for bringing on premature labour, in instances of contracted pelvis, the effect of the equal distension by the gas liberated from three or four ounces of the ten-volume solution, would, I suspect, be as rapid as it would be safe and effective.

Use as Spray.

I reported in my last paper that taking advantage of the ozonic qualities of the peroxide, I introduced it, in 1865, at the meeting of the British Medical Association at Chester, for deodorisation and disinfection of the air of rooms in which patients suffering from infectious diseases are under treatment. I employ it in these examples in the form of spray, and I know of no agent of the kind that is so effective. It communicates to the air of the room the best characteristic of sea-side air; and it would, I am sure, be of special service in the air of wards containing patients suffering from phthisis pulmonalis. In such wards it could be arranged to be distributed automatically, at particular and fixed hours, by means of a clockwork that would turn on a tap for a few minutes, and close it when the proper quantity of spray necessary for diffusion was distributed. The same plan could be adapted to surgical wards; and in private practice it might be applied to the sick room. For these purposes Messrs. Krohne & Sesemann have invented a most effective spray producer, which works at a moment's notice and for long periods diffuses the spray, through a room or ward, in finest state of distribution.

SUPPLEMENTARY CLINICAL NOTES.

The interest taken in the use of peroxide of hydrogen in the treatment of disease has led me to add one or two supplementary notes before proceeding to the pharmacological chapter, with which I propose to bring this essay to a fourth and concluding part in next ASCLEPIAD.

USE IN A CASE OF PLEURO-PNEUMONIA.

During the immediate past quarter I have had under my care a medical friend suffering from acute pleuro-pneumonia. The attack was most severe, was attended with 5° F. of fever, and passed on to the formation of circumscribed pleural abscess, with rupture of bronchial tube, expectoration of purulent matter, and all the usual concomitant symptoms of purulent formation, febrile excitement and exhaustion followed by free perspiration. At this stage I prescribed the use of the peroxide, by inhalation, in combination with anhydrous ether. To carry out the intention, I fitted up an apparatus consisting of a pint globe of strong glass, having two necks. Into one of these necks a glass funnel, armed with a glass tap, was inserted. Into the other neck I fitted the end of an india-rubber tube, four feet long, and three-quarters of an inch in diameter, terminating in the old double valved chloroform inhaler mouth-piece, commenced by myself, improved by the late Dr. John Snow, and completed by the late Dr. Francis Sibson. The glass globe was charged with ten ounces of the thirty-volume solution of the peroxide prepared by Messrs. Robbins, of Oxford Street. On the surface of the solution two fluid drachms of anhydrous ether were poured, so as to form a film of ether over the surface of the solution. When the administration had

to commence, two ounces of a solution of permanganate of potassa, containing five grains of the permanganate salt to the ounce of water, were put into the funnel and allowed to pass gently into the peroxide solution beneath. As the two solutions came into contact and commingled, oxygen was freely given off, and passing through the ether, yielded an ethereal atmosphere, which the patient readily inhaled from the mouth-piece. Both the chemical solutions, the manganese and the peroxide, supplied oxygen, and the liberated gas in passing through the film of ether, a part of which it carried away with it, was ozonised. The gas was pleasantly ethereal to inhale, and very soothing for the cough, acting in fact after the manner of an anodyne. The patient quickly learned how to administer to himself the proper dose, one single lesson in administration being sufficient to show to him two tests by which it is known that the supply of the oxygen is exhausted, one test being the cessation of the taste of ether, the other the discoloration of the bright red permanganate solution. He administered the gas to himself in the quantities named above every four hours at first, reducing the number of administrations after the third day, to every six hours or eight hours according to symptoms until it was no longer required.

The action of the inhalation in this case was without exception the most satisfactory I have ever witnessed. Under it the cough was rapidly controlled, the excacerbations declined, pain was relieved, and the expectoration of pus ceased, with disappearance of the physical signs of purulent exudation. We had naturally dreaded extensive empyema, the necessity, perhaps, of operation of tapping the chest, and a prolonged recovery. Happily, all these anxieties were saved, and in a few weeks convalescence was complete.

Use in Gonorrhœa.

I had occasion during the past quarter to prescribe the solution in a case of gonorrhœa, at a time when a troublesome purulent discharge was passing into gleet. A variety of injections, including one of nitrate of silver, had been employed, but without any controlling effect on the discharge. The injection I prescribed consisted of the solution of the peroxide (ten-volume strength and of neutral reaction), one fluid ounce; tannin, ten grains; distilled water, three fluid ounces. To make an injection of four fluid ounces, of which half a fluid ounce was to be injected twice daily. Within a week the discharge had entirely ceased, and now, ten weeks later still, has not returned. The patient said that the first injection caused a little smarting, followed by the escape from the urethra, as he withdrew the syringe, of some frothy and rather "sticky" fluid; but afterwards the fluid became less tenacious, more watery, and at last seemed to consist of the fluid he had injected only, evidence, as I took it, that there was no remaining pus, and that further injection was, therefore, unnecessary. The correctness of this view was confirmed by the result.

Some Reported Clinical Results.

Several correspondents have written to me during the quarter on points relating to the employment of the peroxide:

Dr. O'Rorke, of Crossakiel, Co. Meath, consulted me by letter respecting a case of *rupture of the vagina* that had occurred during labour, and from which there had ensued much pain and offensive discharge. My correspondent, who detailed the symptoms with the greatest care, was

anxious to know whether I thought the peroxide solution would be of service. I replied affirmatively, and added a word or two as to the best method of application. He carried out the practice at once by employing the peroxide solution of ten-volume strength, diluted with equal parts of water that had been boiled, and charged with five grains of tannic acid to ten ounces. The solution was kept applied to the affected part by means of sponges, and the effect, he reports, was so completely satisfactory that a healthy healing, which commenced immediately, was completed within twenty-four days, leaving a firm union of the lacerated parts.

Mr. T. Edward Williams, of Talgarth, Breconshire, has sent me particulars of a case in which a patient suffering from *strumous disease of the ankle joint*, with several sinuses, was doing favourably under injections of solution of the peroxide of five-volume strength, but who, upon changing the solution into one of ten-volume strength, began to suffer from malaise, with a rash on the skin, and sympathetic swelling in the opposite ankle. Mr. Williams wished to know if I had encountered any similar experience? My reply to this inquiry is that I have seen no such symptoms following upon the use of the peroxide in any form, and that the symptoms were probably of constitutional rather than of local origin. At the same time, the fact should be remembered, with the object of noticing whether under active local oxidation of dead purulent matter near to living tissue any secondary product may be produced which may give rise to constitutional disturbance.

OLD AND NEW PHYSIC.

OLD PHYSIC.

IT would require many numbers of the Asclepiad to describe the changes in forms of practice that have taken place in the past five decades. The aged men even of my first days in physic would not know what to make of the physic of this time. I remember, in the clearest manner, a practitioner of the good old type in my boy-days. I often lived in his house, and the first operation over which I stood trembling was neatly and expeditiously performed by him, in the extraction of a molar tooth with an instrument—by no means so bad an instrument as it has been accredited by the way—called the key. He was a gentleman every inch of him, in education, in courtesy, in dress, in manner. His dress was singularly becoming; it was the dress which in his early life was ordinary in the professions, but fast dying out. It is still seen, in part, in the dress of the serjeant-at-law, and perverted and vulgarised in some ecclesiastical costumes. The coat, of dark cloth, was single-breasted, fitted neatly to the body, and coming well round the chest was cut away something like a modern dress-coat, but without the absurd right angles which separate the body of the modern garment from the tails. The waistcoat, of black silk, was rather long, the pockets of good size, with nicely

cut flaps, and with an open front, from which issued a fine cambric frilled shirt-front, surmounted by a white neckerchief. The nether garments were dark knee-breeches of good fit, with what a Glasgow student, who afterwards stumbled into a baronetcy, was accustomed facetiously to describe as "the rummy fall down with the artful dodge at the knee;" but, nevertheless, they suited well. On special occasions of ceremony the breeches were continued by black silk stockings, with light shoes, nicely fitting, mounted with broad silver buckles; but on ordinary occasions they were finished off with black gaiters and heavier shoes, and, for riding, with long boots of black leather which extended up to the knee. To complete the outdoor costume, a low and rather broad-brimmed hat covered a head the hair of which, curled in front, was neatly brushed back. Taken altogether the dress was in excellent taste; it gave to its wearer a distinguished appearance, subdued when compared with the be-wigged and fuller costume of a still earlier time, yet contrasting strongly with the any-sort costume of to-day. The dress was not peculiar to the doctor; it was what was called the habit of the gentleman of the period; and only marked the gentleman of the medical faculty when he carried with it the gold-headed cane, a cane containing a hollow box at its top, perforated like a pepper-box, from which its owner could breathe into his nostrils the odours of camphor or other supposed antiseptic substance, during his attendance on the infected sick; for in those days Dwight's animalcular or vital theory of contagion, the germ theory of our time, was still in force.

In his method of treating the sick, the practitioner of the old school had a lively sense of the value of medicines and of remedial substances generally. He cared little or nothing about hygiene. He let his nurses draw down the

blinds of the windows of the sick-room, and make it a dark and dirty dungeon, without the slightest compunction. He did not object if the excretions from the patients were left in the utensils under or near the bed for hours at a time. He had no love for open windows; and, unless the weather were very cold, he never troubled about the fire or the external temperature; but he enforced freedom from noise, and secured mental repose by directing the street below to be covered with straw. He ordered low diet in acute cases; and, as a rule, he knocked off stimulants so rigorously that his permission to administer a glass of wine, or beer, was looked upon by the joyful family as a first and certain sign of recovery, a practice out of which Dr. Cheyne, of Dublin, made fine satire. He never for a moment forgot his potent remedies. The lancet, the cupping-glass, the leech, the calomel pill and black draught, the effervescing mixture to be taken every three or four hours, the sleeping draught, the cooling lotion to be applied to the head, the blister and the blister ointment, followed by the "tonic" day after day "during convalescence"—these were almost inseparables from his routine. What in my young days I myself have had to dispense, in this fashion, perhaps for one unfortunate man or woman, is too sad to remember too faithfully. We had our medicine boy, who took out the physic in a basket with two lids opening from the centre. One half would take about "six lots," and one small boy would deliver both halves—a good load—in one round, while in busy times we would press the postman into our service, to relieve the boy or boys.

Say what we may of him, now, the doctor of the old school was right loyal to his remedies. He had at his command fearful means, and he stuck to them. They were not many nor much varied, but such as they were there was no mistake about them. Tartar emetic in

sixth-of-a-grain dose until it was "tolerated;" mercury until it "touched the gums;" half an ounce or even an ounce of Epsom salts; effervescing tartrate of potash or soda mixture *ad libitum*; bark up to cinchonism; and occasionally a bolus!

But the great remedy was blood-letting: every man carried a lancet; and a tortoise-shell lancet-case holding two bright blades was considered the most befitting present for a youth about to be articled. Mine I keep still as a *souvenir*. It was a joke to direct a youth to learn to practise venesection on what were called "the veins of a cabbage leaf;" and some, thinking it proper advice, did, in their innocence, begin in that style. The lancet, in frequent use, was supplemented by the leech and the cupping-glass. It may seem incredible, but it is the fact, that I knew of one practice in which the leech bill alone reached the sum of one hundred and fifty pounds a year. I also knew a practice in which a man, who had learned to apply leeches skilfully, was attached to the firm as the regular "leech-man." He—dear old fellow!—was too good and devoted to his work ever to be forgotten; he could make leeches bite when no one else could, "they loved him so;" he was known to fame the country round, and many a countryman was ready to swear that he owed his life to "Old Josh," the "leech-man." As to cupping, that was an art of itself, practised by the professed cupper. I remember a firm of cuppers, father and son, in London, who lived and saved money by their art. One of them, when I was working on the subject of the coagulation of the blood, allowed me to fit up the glass cups he used with slides inside them charged with acids in order to fix the ammonia which might be given off from the blood in vapour. This was Mr. Betts; he rendered me efficient as well as kindly service.

There is no wonder that, in the treatment of acute disease, medicine was not eminently successful in the old days. The wonder is rather that patients recovered so well as they did. Dr. Robert Willis told me that the famous Dr. Gregory, when piling on the "heroic treatment," would say to his students, "Don't be alarmed, gentlemen; it takes a great deal to kill a man." Gregory was right, and if some were not really killed it was more by good luck than good management. It was not successful treatment, as all who have lived to compare it with our reformed treatment admit. It was bad at the time, for it left many bad after-effects, and it was far worse than no treatment at all of a medicinal kind, a fact the discovery of which every honest observer must in justice accord to the schismatic School of Homœopathy.

The heroic treatment was in swing for nearly a century; and, although John Brown and his disciples gave it a check which might have been its death-blow if he had been as sober as Hahnemann, it continued after him over half a century, and then died hard.

He would be both a bold and mistaken man who would defend the old school of physic in the ways it followed, as above faithfully described, and yet in many other ways it carried itself in admirable form. It cultivated surgery to perfection, and anticipated, in the most practical manner, the treatment now pursued of closing wounds so as to exclude air. Belloste of Paris, in his essay on the cure of wounds by exclusion of air, revived the old mediæval practice of wrapping up every "green wound" in Friar's balsam and leaving the dressing untouched for fourteen days, and he had many followers. Mr. John White, surgeon to the Dowlais Iron Works, treated all his surgical cases, including the severest wounds and compound fractures, in this way, in

combination with apposition of parts and perfect rest; and in the domestic school where I learned "the rudimans" the same mode of treating wounds was always followed, and has never been departed from in my own practice, except when I added to Friar's balsam a styptic, tannin, and made it more protective by collodion, in the form of styptic colloid.

The men of the old school learned well and perfected the rules of obstetric art until they made themselves masters of it. They cultivated botany with devotion and made it a science; they cultivated natural history and laid the foundations of our modern systems of geology, zoology, and microscopy; they sustained and improved anatomy; they invented chemistry in most of its great departments; they first used galvanic and voltaic electricity; they classified diseases in proper order and nosology; they presented us with the stethoscope for diagnosis; they founded forensic medicine and medical jurisprudence; they began to work at anæsthesia; they kept alive classical and scholarly medicine, leaving Adams and Greenhill as the last splendid specimens of their erudition; and a few of them, like Short, Pringle, Marx, and Tardieu, were early fathers of the modern hygiene. It was only by heroic treatment—in certain details of which, especially in the matter of blood-letting, there was after all some good—that they fell. No! there was one fall more. Seduced by the enthusiasm of a handsome, noble and clever woman, they committed the consummate error of trying to wipe out one of the great plagues of mankind by inoculating healthy people with its virus and transforming it systematically from an endemic or sporadic evil into such an epidemic and universal manifestation that almost every human dwelling became its nursery and nesting-place.

NEW PHYSIC.

The contrast between new and old physic in matter of practice is striking. We perpetuate few of the old errors, but we invent new ones for ourselves; we retain many of the old virtues, and we improve or add some by which the signs of such progress as we have made are indicated. In therapeutics the change is phenomenal. Our fathers followed Galen in the view that every practitioner ought to hold all his remedies in his own hand and dispense them himself; the chemist and druggist was the demon. Now it is considered too commonplace for the doctor to dispense his own remedies, and when he does so it is as a sort of favour or necessity —something thrown in, to form, incidentally, or not at all, an addition to the fees for attendance. The doctor's boy with the two-lidded basket is becoming as defunct as the doctor's bill with the red-inked money columns on long sheets of bill-paper. The art and mystery of the apothecary has passed over to the pharmaceutist, who pockets the art and kicks out the mystery. This modifies our prescribing altogether. The pharmaceutist pushes his trade industriously to meet our wants, and our wants are many. He grows bolder; he invents for us in advance of our needs. He floods our breakfast-tables with his nostrums, advices, and temptations. He is a professor of materia medica, pharmacology, hygienics, diet and regimen, all rolled into one. He has a new remedy for every day in the year, with two for holy days, so that any remedy that lives for a year has a long life. To the one grand narcotic of the old school, opium, so trustworthy, so obedient, so safe, when correctly mastered, there are now a hundred rivals, and not one its equal. The result is that a large section of practitioners is flying after everything, trying

everything, and holding fast by nothing; whilst another smaller section is giving up everything, or, in state of greatest activity, is playing placebo with considerable luck in the play, to their own astonishment.

As with medicaments so with practices. In the good old days of physic the great men were universalists, and the best universalist was the most approved as being deep in his knowledge and as bearing the true title to confidence and commendation. Let a man show a taint of specialism and down he went. "All a specialist, half a quack," was a saying of the late Sir Benjamin Brodie, which represented thoroughly the transitional views of his time. But for several years past specialism has taken such hold on the public mind, that, perforce, the universalists have been almost driven to haul down their colours. The profession is now like a Japanese box, one box within another for any number of boxes—the largest and outer one the general practitioner, who, although he covers the whole, is often carefully set aside by the sick person, in favour of a more restricted one within. Some particular persons seem to have as many specialists as they have organs. I know a lady who boasts of her eye doctor, ear doctor, chest doctor, heart doctor, brain doctor, nose doctor, and womb doctor, as well as what she calls her "general prac."; and of all of them she speaks as if they were men of different professions, just as she might speak of her watchmaker, her bellows-maker, or her undertaker. If she could only whip out her eye, or her ear, or her heart, and send it to the right man to be repaired, she would do it in a minute. She goes to him herself simply because she can't help it. Some of these particularly acute persons run specialism finer still. One of them carries a "hussif list" of doctors, in which she has written out for her friends, as

well as for herself and family, what every man "is clever for" down to a nicety. I got a look at the list, and found my good friend Dr. A. booked as "very clever for the upper part of the apex of the right lung," and Dr. B. for the "lower part of the upper intestine and the neck of the gall-bladder." The utter idiotcy of this kind of thing is at last causing a reaction towards universalism by the best minds; but it will take a long time for the great river of medicine, subdivided now into so many little streams, to get back again into its once noble course sailing with the ages. Perhaps the best way will be to follow the example of one of the ablest brothers amongst us, and become a specialist in everything; universal specialist, holding a new degree,—Doctor Omnium Specialorum.

Whatever sound basis there is for specialism is mechanical, and in medicine there probably will always be a special demand for those who are most skilful with their hands and most delicate in the use of instruments. But in the old days, when specialism was at a discount, there was no lack of excellent surgeons, of men who, in much more trying times and scenes than ours, were ready for every operation that science approved, and for every emergency; a fact showing that fashion, together with an insane tendency to specialise everything and everybody, now rules, rather than pure necessity. To this point in medicine we have at last descended, and having got to the bottom without finding the truth we expected to find, we are, we may hope, groping back again towards the light.

NEW AND OLD ETIQUETTE.

"Old etiquette and new fashion" would have been another appropriate heading to this section of the present

paper, for what old physic would have pronounced etiquette is now mere common outdoor courtesy. In the old days there were distinct classes of practitioners —general practitioners and consultants, the consultants being Fellows of one or other of the Royal Colleges of Physicians or Surgeons. To some extent this rule continues, but in a vastly modified and perhaps better form. Grade has pretty well gone out. The "Fellow of the College" in the past was the consultant in the strictest sense. In consultation he was like the lovely rose of Waller's song, he suffered himself to be admired, but he never put himself forward for admiration. The general practitioner took his patient to the great man, held a *bonâ-fide tête-à-tête* with the most studied ceremony, and all was over. With less ceremony the same may take place still; but in this day patients, without a word to their regular attendants, rush to the consultant, or, as they say in the flattery of their words, to the "fountain head" direct, and the fountain head will now receive them—it is as awful as it is true—alone. Sometimes a patient will go round to five or six fountain heads on the same morning, without telling any one of them that he has already gambolled under another; and having got home with a prescription from each of the heads, will be guided by the opinion of his regular adviser, the chemist and druggist round the corner, as to the prescription most likely to do him good. Sometimes the physician will worm it out of a patient that said patient is under the care of another practitioner, and will write a note to the practitioner about the case. This, which is *en règle* according to the old severe etiquette, is now likely to cause lively offence, however delicately it may be done, both to the patient and the practitioner. In the old time in writing a prescription the physician wrote it and signed it, with his initials; the surgeon signed

with his name in full; the general practitioner received the document without signing it. Now all sign it, usually with initials only, and any one of them may actually write it. In the old time it was strictest rule that in entering the sick-room the practitioner in attendance should lead the way, and the consultant follow, but that on leaving the room the consultant should go out first, and the practitioner in charge follow. This rule was so absolute it became automatic, and a few years ago I should have felt it quite a serious matter to witness a breach of it. In modern physic the rule, still a good one, is, like other rules, out of ordinary consideration. I notice that young practitioners, who have only passed through hospital training, do not recognise it in the least; but, as in ordinary society, invite the consultant as a senior or a stranger to lead the way throughout.

In brief, we have strayed from history, from systems of grades, from severe etiquette. We have revolutionised therapeutics, and lifted away many evils, without however substituting much that is, as yet, so demonstrably scientific as to admit of systematic repetition with assured results. We have become animal engineers, and have found that to put foreign elements into our engine is not such good science for keeping the engine in working order, or for preventing it from falling into decay, as prescribing pure air, pure water, pure food, proper rest, and proper exercise. In these directions new physic contrasts favourably with old; and in them is its work for the future, if it is to retain its place in a wiser world.

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."

BENJAMIN RUSH.

MEMORY AS A TEST OF AGE.

MEMORY is often a good test of age. When a person begins to find the recollection of current and recent work failing, and when he finds the recollection of events of the early part of his life acutely perceptible, and by a kind of spontaneity recurrent, the evidence is certain that the mind of that person is aging. The fact is still further emphasised if, with the remembrance of past days, there is a sympathetic response calling forth a sentimental feeling either of pleasure or of pain. There probably is a physiological reason for these phenomena. In early life certain centres of the brain are filled with impressions and images which have become fixed, and for a time quiescent. They sleep. While they sleep other parts of the brain are charged with new impressions which remain in activity, provoking the physical body into new and continued actions, and constituting the life of the individual as it is seen at work, nay, as it really is. But time goes on, and under the active life the brain centres receiving the later impressions tire, wear out, and for working purposes suspend function and die. Their suspension is not, however, the suspension of the whole of the

cerebral organism. The parts first impressed and imprinted—the parts that carry the latent impressions —remain intact, and no longer oppressed and obscured by that which has accumulated upon them, begin once more to live and display their activities. So aged people who forget the names of those who are staring them in the face, who forget the details of the last ride, or walk, or work, and who forget engagements, letters, and hours of meals, remember with the freshness of youth, the friends of their youth; the places, habits, conversations, events, that have long since passed, and have been so long in oblivion.

The study of memory in relation to age is full of practical as well as of philosophical importance. It bears on the value of evidence of observed facts and phenomena at different stages of life. There are thousands of persons who could give no evidence worthy of credence respecting sayings and events of to-day, who could still give the most accurate and reliable evidence about sayings and events of fifty years ago; and, if sympathies change with memories, there is an explanation, clear enough, why with age likes and dislikes should undergo the astonishing modifications we so often witness. I was called once to see a dying man who was advanced in life. He was muttering something strangely. "What is it he says?" I asked. "I do not know, sir," replied the nurse, "but it's all about Monday, and see how curiously he moves his hands." I listened attentively, and soon caught the words, repeated many times: "Oh Jesu, Agnus Dei, qui tollis peccati mundi. Miserere nobis." I observed thereupon to my medical brother, whom I had been called to meet in consultation, "He," the patient, "is saying part of a

Romish litany; he is a Roman Catholic." "Impossible! I have known him for thirty years, and he has been a man of the freest thought, good in every way, but allied to no creed whatever, and opposed to the Roman Catholic faith." "That may be, but in his early life, I warrant you, he was brought up in that faith, and learned its services." On inquiry my conjectures proved correct. In the first five years of life he was trained in the Catholic ceremonial, since which age he had come under influences that had changed the whole tenor of his thoughts.

The point I wish to make in concluding this opusculum is, in the strictest sense, practical and medical. Whenever a patient, who has passed the fifties, or is fairly into them, reports that current memory is fading and past memory is reviving, and when he reports also that his sympathies are running with his memories, his current sympathies declining, his old ones re-awakening, he is in an indifferent condition. He requires immediate mental rest of those faculties that are becoming impaired, and is in want of pursuits and scenes that will bring new faculties into play. Fortunately we never use up a tithe of our brain surface. There is always ample uncharged surface to work upon even late in life, and if the brain be not physically diseased, new memories may be called forth which open up new activities and cover in the old. William Harvey, in his latter days, took to mathematics, and for the first time followed them with ease, much to his quiet. I knew an aged man who took, under the same circumstances, to music, and became quite a fair violinist; from all which comes a lesson:—

> In second childishness child life revive;
> Learn something new each day, and so re-live.

THE PHYSICAL ACTION OF ODOURS.

THE direct action of odours on the nervous centres is a subject worthy of careful research and study. Goethe had a strong dislike to the odour of apples; Schiller liked the odour. Some persons are made absolutely ill by the odour of onions that are being cooked; whilst other persons rather like it. The odour of the lily has a most potent effect in many instances, and I believe there is no person on whom it does not produce a sense of depression and nausea; I have known it cause positive faintness. I am myself always disagreeably affected by the odour of carbolic acid, and can never remain many minutes in a room where a trace of it prevails. In cases where the effect of an odour is instantaneous, it is fair to suppose that the impression made on the olfactory surface is transmitted direct to the olfactory centre of the brain; but there must also, in certain examples, be a further transmission to the sympathetic ganglia.

The central seat of the olfactory sense must be very near to the central seat of memory, for it is noticeable that nothing recalls a past event like an odour. A little child was accidentally thrown out of a pony-carriage in a country lane. Near the spot where the fall took place there was a manure heap, which gave forth the peculiar dry ammoniacal odour so often recognisable from such heaps, an odour distinctive yet not altogether unpleasant. The child was stunned by the fall, and on recovering and returning to consciousness smelt this odour powerfully. Over fifty years have elapsed since that little mishap, and yet whenever the person referred to passes, in country lanes, a heap

giving out the same odour, the whole scene of the accident recurs with every detail perfect, and sometimes with a recurrence of the giddiness and nausea which were experienced at the moment.

In some of the lower animals memory by odours is often singularly exhibited. In the dog the memory by odour seems a special part of the nature of the animal. The "scent" of the fox-hound and of the stag-hound is of this character. In the trained collie the remembrance of an object hidden, a stick, for instance, may be retained for three-quarters of an hour, so perfectly that the animal will fetch the object at command. But if the object be coated with something giving an odour which the animal is familiar with the time is infinitely more prolonged.

Some odours lead to sleep, like the odour from dried hops; others lead to wakefulness, like the odour of dead flowers or leaves. Still others allow sleep but provoke the most terrible dreams, like the odours arising from a pillow in which feathers are decomposing.

Habit modifies the effects of odour. Merciless smokers laugh at the "faddery" of women who become faint if a smoker charges the air they breathe in a confined space, a small room or a railway carriage, and are ready to compare the objection of a lady unaccustomed to the odour from the pipe or cigar, with the carelessness on the matter shown by another lady who has become accustomed to the effect. But if a smoker gives up smoking and all contact with smoke for a few years, he is astounded at the unpleasantness of an air charged with smoke when he is then enclosed in it. I was once summoned, professionally, to a youth who was temporarily poisoned

by inhaling the atmosphere issuing out of a small window of a club-room in which a number of men were smoking freely. They, in the body of the smoke, were not perceptibly affected; he, partly in the open air, was positively smitten to faintness by the empoisoned current from the room which flowed out of the window, and is still affected whenever he comes within the cloud of a pipe.

ANIMAL TEMPERATURE AND FOOD IN DISEASE.

THE *Lancet* of July 27th ultimo, brings forward the question whether the animal temperature is reduced by change from an animal to a vegetable diet, or to a diet in which animal food forms a main part. It refers to a gentleman and lady who, under what is called the V.E.M. system (vegetable, eggs, and milk), seem to have brought down their animal warmth from 98° to 96°, with 97·4° Fahr. as a maximum, and at the same time have remained in perfect health and strength. If this be true, it is argued, the assumed natural standard of the *genus homo* is above the required standard, and men and women are wasting their powers by an unnecessary dispersion of energy. It is suggested that we ought to ascertain, from a long, patient, and truthful series of observations on the temperatures of animal and mixed feeders, whether by changing them into pure vegetable feeders or fruit feeders, any modification of temperature is induced. The idea is a good one, but the research should be extended to observations on the effect of dietaries in the course of disease. We have no system in the treatment of disease, of febrile disease especially, that so much as

touches this all-important matter. In high fever we give cold drinks, with broths and beef tea, guided more by what, from very crude ideas, we are led to think the stomach will bear, than by any forethought of what the substance, supplied as food, will do when it passes into the circulation. It is not known, elementary as the question is, whether the imbibition of cold water reduces temperature more decidedly than hot water; and when we come to foods, we have no sure knowledge whether those which are animal and fleshy, or those which are farinaceous or those which are fruity, are the most active antipyretics. Perchance there might be discovered some food and drink that of itself would be sustaining and antipyretic. I am usually led by what is called the "instinct" of the patient in directing foods and drinks, and my late friend, Mr. Thomas Hunt, a shrewd and original observer, wrote once an essay to prove that instinct was an infallible guide for food in disease. It is a doubtful doctrine, but possibly up to date as good as any other, if not the best.

MOLECULAR CHANGES IN NERVOUS STRUCTURE.

FOR the future of physic we require to revise our views respecting the molecular changes which occur in nervous matter. The discoveries, in electricity, of Galvani and Volta, and the experiments made by Aldini, the distinguished nephew of Galvani, at the commencement of this century, were sufficient to startle every mind, and to develop a new era of thought. In 1803, one John Forster, a malefactor, twenty-six years old, was hanged at Newgate on the 17th of January, a cold, frosty day.

The malefactor swung in the cold air one hour, with the thermometer two degrees below freezing point. Then his body was conveyed to a house near, and in pursuance of sentence was delivered to the College of Surgeons. Master Keate, Master of the College (some of us remember Master Keate very well), Carpue (Thomas Hood's own Carpue), Hutchins (one of Carpue's prosectors), Cuthbertson the electrician, Blicke, an anatomist, Dr. Pearson, a physiologist, and young Mr. Brodie, were all at this house, together with Aldini. Aldini had a battery of forty cells in three troughs, and malefactor John Forster, cold, stiff, and stark, was subjected to the influence of the battery. An arc was made from the ear to the lower part of the trunk, and as the electrical stream flowed and penetrated into the life-suspended muscles, those muscles played again. John Forster grinned horribly at his manipulators, as if they were hurting him; he opened one eye, and fixed it on something; he moved his limbs. They withdrew the electricity, and John Forster was quiet again; they tried if strong ammonia to his nostrils would influence him, and found it would not; but they re-applied the electricity with the ammonia, and the effect was so extraordinary they thought the wretch was actually alive again; but they stopped, and he stopped. Then they opened his chest and exposed his heart, to find that no electrical current would restore its rhythm; so it was clear that all through the experiment John Forster had not lived by his heart. It is also clear that voluntary muscles may be irritable, while the involuntary heart is quite dead.

The experiment, as well it might be, was the marvel of the world, and Aldini, who did not, he tells us, mean to bring the malefactor back to life, became the hero of

the hour. He was "presented." Master Keate made a good stride toward court eminence, and altogether there was popular fame on the winds travelling briskly over John Forster, malefactor, in 1803. As to the world of science, it was wild with commotion; a volcano bursting through a tranquil lake were not more grandly disturbing. Other experimentalists performed the same experiments on dead malefactors, and with like results; Galvani's theory of animal electricity recovered from the attacks of Volta; and by a vast leap of learned speculation, the human body was declared to be an electrical machine. Of course, for is not the torpedo such a machine, and is not that proof direct? So at once the old researches, from the time of Sylvius, through Haller, Winslow, the Munroes, about the existence of a veritable nervous fluid, went to the wall without question, or were as ignored as if they had never been.

Galvani's and Aldini's experiments were astounding, and rightly read they retain, as do all carefully proven facts, a lasting value; but they led to more error than any of which I know. There is nothing in science of nonsense so gross, as the garner of nonsense that has been gathered, up to this very time, on the so-called animal electricity. Incoherency can go no further than it has gone in this direction, while science has not advanced a minute's march in ninety years toward even a preliminary demonstration of the existence within living bodies of a sign of an electrical mechanism, except in the rare cases of one or two specially constructed electrical animals.

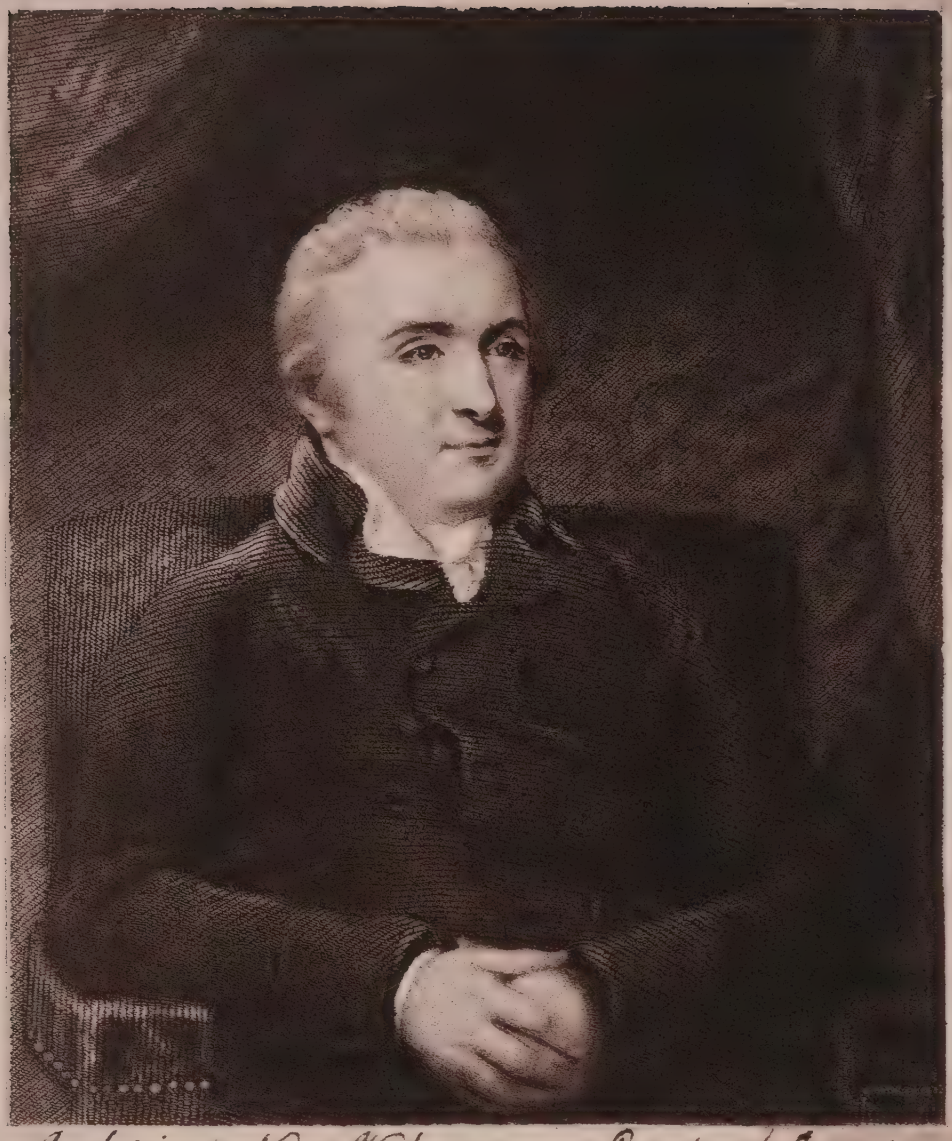
Here then, I think, we have to call back and revise. We want to know, even yet, whether there be a nervous fluid traversing the nervous cords, or circula-

ting between the nerve centres and the blood. And, particularly, we want to ascertain what is the molecular change of matter of the nervous system, when it sleeps or rests, when it wakes or moves. Light, I am glad to say, begins to break on this primary inquiry. We can make nervous substance temporarily solid by cold, *i.e.*, by crystallising it, and then the nervous structure rests and sleeps. We have to see then, whether, when our eyes droop with natural sleep, this same change of structure is not progressing naturally in nervous structure; we have to ask whether under sudden shock—shock from a bullet, for instance—the complete destruction of nervous power is not due to change of nervous matter under sudden vibration of its particles, like the change which occurs when water suddenly solidifies under motion, or when fluid fat becomes a concrete mass under brisk agitation.

METHYL-BROMIDE.

METHYL-BROMIDE, a sharply-acting general anæsthetic, the analogue of ethyl bromide in the ethene series, has lately been, it is reported, the cause of death in several cases in which it has been administered. As it acts with great rapidity, it has gained some favour in dental practice; but, of necessity, with considerable risk. Ethyl bromide, first used by the late Mr. Nunnerley, of Leeds, and afterwards by myself, was discarded because of its danger; yet it is safe in comparison with the lighter methyl bromide. The danger, as I have pointed out more than once, lies in the instability of these compounds. They decompose readily, and may permit free bromine to be liberated during inhalation: an

accident followed by rapid spasmodic contraction of both the pulmonary and coronary arteries, with death by syncope of both the lesser and the greater circulations. Such accidents afford, unfortunately, the best proof of the folly of using any general anæsthetic, until, by careful experiment, the precise action of the substance has been determined. To practise first on human beings with agents so potent as these is the empirical method carried to recklessness, and ought really to come under the correction of law. In the case of the bromides, the administration is all the less excusable in that they have been tried, experimentally, and pronounced dangerous on the most reasonable and clear exposition of their physiological effects. The danger they produce and the cause of the danger has been explained and declared, but it can do no harm once more to declare it. The danger lies in three directions. In the first place, the bromides are unstable compounds. They are influenced by light, by temperature, and by exposure to the air, so that in administering them we can never be sure that we are not administering some portion of free bromine, which, in however minute a quantity, is a source of danger. Secondly, when the compound is fresh and uncharged it is of itself an irritant, and as such is more to be feared than chloroform, more than methylene, and much more than ether. Thirdly, its insolubility is against it. It acts quickly, because it acts immediately on the minute pulmonary circulation; but this very speediness of action is, in its case, its greatest disadvantage.



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M Baillie

Outline from an engraving by H Cook, after a portrait by J Hoppner. R.A.

B.W.R.

MATTHEW BAILLIE, M.D., F.R.S.

MR. PETTIGREW, in opening his life of Dr. Matthew Baillie, quotes a saying of the great Samuel Johnson, that a physician in a large city is "the mere plaything of fortune; that his degree of reputation is for the most part casual; that they who employ him know not his excellences; they that reject him know not his deficiencies." To all of which, true in this day as it was in that of the great moralist, he, the moralist, might have added that neither the successful man nor the unsuccessful has any clear idea why he is what he is. The successful man cannot place his confidence on his superior knowledge; the unsuccessful need not place his failure on ignorance. Neither is it manner, as some suppose, nor appearance of a personal kind, that commands success; for some of the most uncouth creatures have won their way, and assuredly some of the ugliest. The philosophers say that there is no such thing as luck, and we ought to believe them. Why, then, do some men of physic succeed in popularity, and others—their equals or superiors—not succeed? Let us have another saying from the same sage: "Sir, it is vain to try to explain the inscrutable."

Matthew Baillie was one of those who succeeded in physic; one who stood as the most popular men of his day. So says his friend Pettigrew; so says Wardrop, another of his friends and biographers; so said often the

late Robert Willis; whilst another admiring contemporary —Mr. John Fernandez Clarke—was so enthusiastic about his popular glory, he told me "that not only was there never such a favourite physician in London, but there never could be a second like unto him."

Matthew Baillie succeeded? Compare him with his contemporary, Dr. Thomas Young, who never could succeed; he mortal, Young immortal, yet both equally learned in physic; and again ask the reason of success and no success. Again the answer: "Sir, it is vain to try to explain the inscrutable." There may be one special reason why Baillie succeeded in physic. He came in with the Hunterian tide, and because of this stroke of good fortune I put him forward here. Both William and John Hunter, of whom we have learned so much, made the way for him. They were his forerunners, his mother, Dorothea Hunter, being the sister of John and William Hunter, whom we know as having married the Rev. James Baillie, minister first of Shots, in the county of Lanark, then of Bothwell, afterwards of Hamilton, and finally Professor of Divinity in the University of Glasgow.

The March to Fame.

Baillie was born at the manse of Shots on October 27th, 1761. He grew up under the best influences; took his education in the Grammar School at Hamilton, and, when he had reached the proper age, went to the University of Glasgow and joined its red-robed students' corps. There he gained an Exhibition for Oxford, soon after the first sad event of his life—the death of his father. The Exhibition obtained, he lost no time in availing himself of its advantages, and on March 18th, 1779, was ready to start southwards. His uncle—William Hunter—by this time widely famous in his School of Anatomy in

Windmill Street, London, was some one for the youth to rely upon; and so we find him sending off a preparatory letter to that distinguished teacher. The letter breathes a fine spirit, and could not fail to win its intention. The road to Oxford is conveniently through London, and the advice of the wise and experienced uncle, will be to him of great value. What shall he do at the University? what are the manners of the place? He hears that there is a great deal of dissipation at Oxford, and he would, therefore, wish his uncle's warmest advice in regard to his behaviour. Then in the tenderest manner he speaks of his mother and sisters. He has prevailed on them to stay two or three days in Glasgow before his departure, that they may be diverted from reflecting too much upon it, all of them being very susceptible of impression. He would like to make the parting as light as possible, and he hopes the consideration that he is going to a person who will protect him so long as he deserves the protection, will render this far easier than otherwise it might be. "Accept," he says, in conclusion, "of everything a grateful heart can give. I must confess I am in some measure afraid to appear before you lest my progress should seem much inferior to what might have been expected; but I trust much in your goodness, that you will make every reasonable allowance for these deficiences which may appear."

If this appeal to Uncle William were not sufficient he carries with him another, which of a certainty must bear all before it. It is a brief note from the sorrowing mother to her brother: "I have furnished him out in the best manner my situation could afford. I now give him over to you. Be a father to him—you the only father he has alive. I hope you shall never be ashamed of his conduct, but that he shall obey your directions in everything."

It is curious to observe that in this motherly communication no reference is made to brother John, who by the period named was making his way into fame. William evidently was the reliable host, who was to receive the young man under his care and guide him in the right way. Perchance the story of brother John and the wild oats he sowed for a time so industriously had reached the ears of the thoughtful widow Baillie, and made her select the more trusted guardian for her son.

The Hunters were a united family, and young Baillie met with all the consideration he expected from his uncle, who, now advanced in years and unmarried, would naturally find in him a companion after his heart's desire. There was still some energy of life in the old man, a fund of story and anecdote which made him the delight of the table. He had the widest knowledge of men and things, and as a teacher was as perfect as a teacher could be. In manner he was altogether unlike his rough and impetuous brother John. He was refined, eloquent, and most prudent and careful in regard to means. Everybody said he was rich, a saying John echoed in a note with which he introduced a poor patient:—

"Dear Brother,—The bearer is very desirous of having your opinion. I do not know his case. He has no money, and you do not want any; so that you are well met. "Ever yours, John Hunter."

The Student.

Thus, in the person of William Hunter, all combined to make the introduction of nephew Baillie a stroke of good fortune, if we may not say luck; and widow Baillie, shrewd woman, knew for the best. The nephew took. He found in William Hunter an affectionate as well as a

munificent patron, and he formed in the society of Windmill Street a number of important friends. We can easily receive, as strictly exact, the statement of Wardrop that his communion with Uncle William was "delightful." After a sufficient residence with his uncle to make acquaintance and gain favour, Baillie proceeded to Oxford; but his connection with the University was almost nominal. There was no medical school there, and the son of a Scotch Professor in a good Scottish University, who had already gone through a full course of academical study, would not require much further instruction preliminary to an active career in anatomy and medicine. For about a year he remained in Oxford, coming up during vacation times to join his uncle in Windmill Street work, and learn his future profession; and, after one year, he left the University and returned to London for good. He acquired, however, at Oxford, a good reputation as a studious and diligent youth; one of whom his tutor, Dr. Prosser, afterwards Archdeacon of Durham, spoke with great regard. Here also he made many friendships, which were of service to him in his after career.

In Windmill Street, he went through the probationary period in anatomy necessary to become a teacher of anatomy, and he soon became a fitting successor to the master of the school. He was assiduous in his duties; accurate, and, like his uncle, ready in exposition. He had never the commanding grace and eloquence of his uncle, for he was below the middle height in stature, in which respect he was nearer to his Uncle John; and he had none of the vivacity and life of language which makes a man an eloquent, as distinguished from a ready speaker; but he taught thoroughly well, and attracted so much by his powers as a demonstrator, that the school was maintained by him in all its integrity, and continued

to flourish when it was entirely under his own management—an event which quickly followed, for William Hunter, full of years and honour, died three years after Baillie first came to him.

On his uncle's death, Baillie entered into the possession of considerable property. The Windmill Street school and house were left to him; all the goodwill of the school; a grand museum, now the famous Hunterian at Glasgow, for life; the estate of the family in Scotland, where the brothers Hunter were born (Long Calderwood); and the sum of £100 a year for life. Why more wealth was not left him in the form of money, was explained by the testator—"that it was his intention to leave him but little money, as he," the testator, "had derived too much pleasure from making his own fortune to deprive him of doing the same." It reflects the highest credit on Baillie, that he gave up his possession of the estate at Long Calderwood in favour of his uncle, John Hunter, who felt severely the failure of remembrance from his elder brother. The estate itself had fallen out of the Hunter family, but had been repurchased by William Hunter.

For both his uncles Baillie entertained proper respect, and he discussed their relative merits with keen and impartial judgment. Wardrop says of them, that "William Hunter was distinguished for his talents, but John was a man of extraordinary genius." Baillie does not distinguish them quite so affirmatively. He says: "No one ever possessed more enthusiasm for the art, more persevering industry, more acuteness of investigation, more perspicuity of expression, or, indeed, a greater share of natual eloquence, than William Hunter. The mind of John Hunter was bold and inventive, travelling constantly in a path of its own, without regard to the common track which had

been followed by others. This was aided by an industry and enthusiasm of which it would be difficult to find any superior example; with such singular endowments for the cultivation of science, his progress was proportionately great. There is no subject which he has considered where he has not added new light, and there are many which he has very much improved."

The Teacher.

In his twenty-second year, Baillie was at the head of the Windmill Street School. He associated himself in the labour of conducting the school, with a most able coadjutor, Mr. William Cumberland Cruikshank, F.R.S. Cruikshank was sixteen years the senior of Baillie; he had succeeded Hewson as demonstrator in the Windmill Street School in 1771; had won the entire confidence of Dr. Hunter, and knew every part of the labour of a teacher of anatomy thoroughly. He also was much esteemed in practice, and to lovers of the life of Samuel Johnson, will be remembered as the "sweet blooded man" of that illustrious scholar, and his surgical attendant in his last and fatal illness. Cruikshank was a good original observer on the physiological side of anatomy, while Baillie turned more to the pathological. The two worked well and harmoniously together.

If Cruikshank had survived Baillie, the museum of William Hunter would have come to him for thirty years succeeding upon the death of Hunter, but he began at once to make numerous preparations for demonstrations of his own, by which he formed a museum sold after his early death, in the year 1800, to the Russian Government, and removed to St. Petersburg, where parts of it, I believe, still remain. Baillie, on his side, commenced to form a pathological museum; which, in the end, he bequeathed

to the Royal College of Physicians, together with a sum of £400 for keeping up the collection in good order. The specimens amounted to a thousand in number.

The position of teacher in the first school of medicine in England was an enviable one, and Baillie felt the advantages of it. His friend, Wardrop, who became his biographer, says that this was the happiest season of his life; and we may well believe it, for there is really no work so wholesome, bracing and fascinating to those who like it as that of the Professor. It brings a man into contact with those who are young of heart and hope; it keeps foremost the knowledge of the teacher's own superiority; it leads gently to new studies, so that the superiority may be maintained; and it makes delightful friendships all round, with gratitude as the soundest basis of friendships that shall be enduring. In many respects, it might have been better for himself and for science, if no allurement had led Baillie from his original sphere as a Professor. Whilst he continued in it he did good work, which remains behind him, to his lasting honour, in his treatises on morbid anatomy. After he left it, he became, as he himself felt, a mere drudge of the chariot and sick room, grinding the daily grind, an exhausting man, and no longer a contributor to the service he loved best.

He continued to teach almost exclusively until the year 1787, when he took up his doctor's degree at Oxford, was elected a Fellow of the Royal College of Physicians, and became one of the physicians to St. George's Hospital, the institution in which his uncle John was at that time the shining light, light with now and then an unpleasant blaze that sadly alarmed the authorities there, and cost them much anxiety. The result of these changes in his life, all tending towards practice rather than anatomy, soon had their influence: he was becoming a practitioner,

and the least turn more in that direction would be sufficient to decide his fate.

THE POPULAR PHYSICIAN.

The turn came. His friend Dr. David Pitcairn, a man who had gained great distinction as a London physician, who had been a close ally of Dr. William Hunter, and who had exhibited a marked liking for the nephew, began to feel the toils of practice irksome. Baillie thereupon commenced to relieve him of a part of his duties, and, in the end, succeeded him altogether. At first there was some difficulty in the way, for the public who could afford to pay large fees to a physician, were not easily inclined to accept a mere anatomist, however accomplished as such, in the character of a prescribing physician. The members of the profession in that day were not appreciative of the value of a profound knowledge of anatomy in a practitioner of pure physic. It was good and necessary knowledge for a surgeon, but not for practice strictly medical. It was, therefore, a nice point for our friend to determine whether he should retain the substantial income he had secured by teaching, an art which was to him most congenial, or give it up and devote all his energies to practice. The chance of making a larger income at the bedside than at the lecture table prevailed, and the chair was vacated for the chariot. Many influences in favour of the new sphere were favourable. The connection with the Hunters was naturally favourable; the patronage of Dr. Pitcairn was itself a foundation; and in a few years Baillie was at the head of the prescribing practitioners of his day. He was the physician of London, and no one of mark considered that all that could be done had been done for him in sickness unless Baillie were summoned. Fees flowed in to him like a tide, and he who

originally cared, or seemed to care, nothing for filthy lucre appeared, with this flow of fortune, to be bent on nothing so much as in adding to his store. He became, in short, now so accustomed to accumulate money that he accumulated as a matter of course, and worked away for dear riches as if he were working for dear life. Perhaps there were other, and it is only fair to say there were other, impulses in his heart. He was a conscientious man in work, and responded to impressions which had other origins than mere love of gain. He showed these impulses, before he entered into active practice as a physician, in an introductory lecture which he delivered on anatomy. In that discourse he taught a doctrine which even to this day has not been duly recognised, namely, that "a disease must always have a relation to a healthy action, or a healthy structure of parts, for it is only a deviation from them, so that a knowledge of disease would appear to rest on a knowledge of the body in its healthy state." Then, after pleading in favour of a knowledge of anatomy and physiology as an aid to physic as well as surgery, he added a note which sounds well as an indication of the fine and healthy tone of his mind: "This is not a trifling matter. Justice and humanity require every exertion where the lives of our fellow creatures are concerned. There are many professions where negligence or inattention may be reckoned as folly; but in medicine it is a crime. There is nothing that renders a person more fit for the discovery of new diseases than a knowledge of anatomy. Who can be so able as he who is familiar with the natural structure and diseased appearances in an animal body? There are diseased appearances which are common to an animal body, and which are of no great consequence. It requires a familiarity with the body to distinguish them from appearances strictly natural,

or from diseases that are really serious. This last circumstance is of great importance, when we consider how often we are required to examine bodies after death, for the satisfaction of friends or for judicial inquiries. On our judgment may depend the life of a fellow creature. What reflection can be more serious to a man of humanity? But independently of these circumstances, there will be many cases of doubt presenting themselves to our minds which we would wish to settle, but shall not be able, unless we are acquainted with natural structure and diseased appearances. Yet how is physic to be improved otherwise?"

I quote this passage as an excellent and, doubtless, truthful description, ingenuously conveyed, of the tone of mind with which Baillie entered on his practical career as a physician. The bias of his mind scientifically and morally is there set forth. He was rendered by education a student of morbid anatomy, then comparatively a new study. Bonetus and Morgagni had preceded him in this course, but their voluminous works were comparatively little known, and there was about them, and about others of lesser note in the same line, a complexity and, if I may so say, a recrudescence which embarrassed the study as well as the student. It was Baillie's task to lay the foundations of an English school of morbid anatomy, and it was well laid in his famous treatise on the subject published in 1795 and enriched, four years later, by a series of "engravings," in illustration, from the drawings of Mr. Clift, the long and much-valued Curator of that Hunterian Museum to the work of which he was born, lived, and died.

Started on these foundations as a practitioner and, unfortunately for us, closing up his original labours with the close of the century in which he began to work,

Baillie entered on his successful practice with the present nineteenth century, and rose in the first fifth of it to the high popular position which has already been noticed. He moved from his residence in Windmill Street into fashionable Grosvenor Street, and being, by this time, married to Sophia, the second daughter of the distinguished Dr. Denman, he found his social position as secure as his financial. He had two children: a daughter, Elizabeth Margaret, afterwards Mrs. Robert Miligan, and a son, Mr. Robert Hunter Baillie; and, in these first days of his notable career, was one of the most enviable of men. His success made the first inroads on his happiness. Naturally of feeble constitution, the drudgery of practice told upon him with steady encroachment up to positive disablement. The profession learned to depend on him, and no man is so tried as one in this happy, unhappy state. He felt his own deficiencies most keenly of all. "I know better perhaps than another man, from my knowledge of anatomy, how to discover a disease; but when I have done so, I don't know better how to cure it." The confession, true as it was painful, seemed to influence him in the whole of his dealings with his fellow men. Still he laboured on, as strict as John Hunter himself in the keeping of his appointments, and guided by a very clear and concise rule on the matter, which ran as follows:—

"I consider it not only a professional but a moral duty to meet punctually my professional brethren of all ranks. My equals have a right to such a mark of respect, and I would shudder at the apprehension of lessening a junior practitioner in the eyes of his patient by not keeping an appointment with him."

He was not merely considerate in regard to the order of appointments. He was anxious ever to help forward struggling practitioners with as much fairness towards

them as generosity. Wardrop records a pleasing instance of this kind. "When," Wardrop says, "he (Baillie) could afford assistance directly to an individual, he cautiously avoided doing so if it was in any way to the prejudice of another. I remember being told by a person whom he thus assisted on his first entry into life, that some years afterwards, when Dr. Baillie found him fairly established, he said to him, 'You must not expect me any more to recommend you, for it now behoves me to advance the interests of those who are younger, and who stand more in need of my support.'"

There seems to have been but one opinion as to the popularity and amiability of Baillie. As a physician, a man of science, or a social companion, he shone equally. Sometimes he showed a little irritability of temper, so that when he sat down to meals with his family he would, for a moment, bid them not speak to him; but he soon rallied, and, coming back to his usual good nature, would almost apologise, begging them to accept his abruptness as the mere effect of fatigue, and reassuming all his genial character after he had rested and refreshed himself. His labours were incessant, and held him from morning until late at night in continued physical and mental vibration. It mattered little how wearied or how feverish from weariness he might be, he was up and doing; and possessed of a frame naturally feeble, he wore out rapidly. He was wanted everywhere in his sphere of action: in the wards of St. George's Hospital; in the Royal College of Physicians, of which he was one of the Censors, elected first in 1792, and again in 1797; or on Commission for the inspection and licensing of houses for the reception of the insane. Whenever it was possible, also, he would be present at a scientific and social club started by Dr. Russell, one of the Physicians to St. Thomas' Hospital, and sustained

by the presence of such scholars as Sir Gilbert Blane; Sir Astley Cooper; Mr. (afterwards Sir) Benjamin Brodie; Sir Everard Home; Dr. (afterwards Sir) Henry Holland; Dr. Somerville, husband of the famous lady scholar of that name; Dr. Roget; the famous Dr. Wells,—"dew-drop Wells," as he was commonly called—and Dr. Fordyce. Dr. Roget, in company with whom I had more than once the pleasure of dining at the house of a mutual friend, and who through his "Thesaurus" is still a treasured friend, described this club, which he well remembered, as the cradle of his literary life; and his account of Baillie as a kind of "will-o'-the-wisp," who came in and brightened the members up for half an hour, to be off again before they had full grip of him, was very entertaining. "He was always too busy for us," Dr. Roget remarked, "and injured himself by the volume of work which he made his weak body carry on from day to day; for he had the largest practice in London, and he neglected nothing that came in his way."

Above all the duties and pleasures named, Dr. Baillie had indeed, after a comparatively short probation, the largest practice of his day. Pettigrew estimates that his income, from practice, was over £10,000 a year, and that at a time when the one-guinea fee was considered the right thing. He must therefore have seen regularly over thirty paying patients a day, except when engaged in long country visits, of which he took comparatively few and those far between, exertion incompatible with health, and leading, of necessity, to the brief career I shall have to record.

In the midst of his so-called successful life Baillie had an advantage, which, though "aside," as the playwrights say, was to him of moment. So soon as he became famous, his sister Joanna migrated from her and his Scottish

home, made her way London-wards, and settling finally at Hampstead, resided there until her death on February 23rd, 1851, at the age of eighty-eight years. She, like her brother, gained early reputation. Her talents were devoted to poetic and dramatic literature; and although her plays, notwithstanding their tenderness, beauty, and in some instances force, did not take the popular fancy, even when Kemble and Edmund Kean gave them trial on the stage, her character and genius were widely acknowledged during her life, and reflected upon her brother a lustre which he was proud to feel and recognise.

The death of Dr. Warren added still more to the practice of Dr. Baillie, who was called upon soon after this event to attend the Duke of Gloucester, and afterwards the King, George III., the Princess Amelia, and the Princess Charlotte of Wales. It is a curious fact in regard to the King, whose mind, as it is well known, became disturbed, that Baillie considered him endowed naturally with perceptive qualities unusually acute and penetrating. Wardrop informs us that, speaking of the King, Baillie observed, "If I knew anything I wished to conceal, I would rather be cross-questioned regarding it by any barrister in England than by the King, for his questions bear so directly on those points most important for discovery, and are put in such a manner that they cannot be evaded." On another occasion he related the following anecdote:—"One day, when I waited on the King with the other medical attendants, in order to give an account of the Princess Amelia, His Majesty said to me, 'Dr. Baillie, I have a favour to ask of you, which I hope you will not refuse me; it is that you will become my Physician Extraordinary.' I bowed, and made the best acknowledgments, in words, that I could. His Majesty added, 'I thought you would not refuse me, and, therefore,

I have given directions that your appointment shall be made out.' A few days afterwards, when we again waited on the King, he said to the other medical men, in my presence, 'I have made Dr. Baillie my Physician Extraordinary against his will, but not against his heart.' In another instance, recorded by Wardrop, the King was advised to go to Bath, and Dr. Baillie recommended him to consult there a medical gentleman whom he named. The King immediately conjectured the country from which this gentleman came, and, after listening to all Dr. Baillie had to say of him, added slyly, 'I suppose, Dr. Baillie, he is not a Scotchman!'"

Dr. Baillie had the King for a patient for ten years, and during this interval went regularly to Windsor professionally. At first this was a serious interruption to his ordinary practice, but after a while he felt it a relief. In the course of this time the honour of a baronetcy was offered him, which he did not accept. He was more than content with the honours which learned societies, like the Royal Society and the Royal College of Physicians, had conferred upon him in the commencement of his career He was elected F.R.S. in 1789.

BAILLIE IN LITERATURE AND SCIENCE.

Such was Matthew Baillie through the early parts and up to the zenith of his fame as anatomist, physician, and man of science. It is now time that we turn to the works he has left to us, and by which he belongs to us even to this day. The reputation of Dr. Baillie as a man of science and a contributor to medical literature rests entirely on his labours in morbid anatomy. To this department he was trained by and for his work under his uncle, William Hunter, during the short time he was with him, and afterwards in the School of Anatomy in which he laboured. His observations were largely collected in that school,

and it has been observed that after he was fairly installed in practice he added nothing to literature of a new and original kind. His literary life terminated at his fortieth year and by what he has left behind him, on which we are alone able to form a judgment of his powers, we must place him, first, as a morbid anatomist, and secondly, but quite in a secondary position, as a pathologist. To clinical medicine he added little, and to therapeutics nothing; in fact, as we may gather from his own statements, he made no pretence to therapeutical art. He could find out what was the matter with the sick better than most of his profession, because of his knowledge of anatomy; but of treatment he knew no more than the rest of his brethren. This was his own candid estimate of his own capacities—an estimate, perhaps, that might be adopted even in the present day by many a successful practitioner.

In his research as a morbid anatomist he followed to some extent the plan of Morgagni, for of conceptive genius he had no gift. He found a diseased organ or series of organs; he compared it or them with the same organ or organs in a healthy state; he wrote down, in the briefest terms, the differences; and he recorded the morbid changes as best he could from the knowledge of his day, with a short description of the symptoms that attend the morbid condition described. He tells us that his object was "to explain more minutely than had hitherto been done the changes of structure arising from morbid actions in some of the most important parts of the human body," and he was very exact in that which he committed to paper; but, really and truly, he was not, according to our modern notions, very minute. He seems to have used the microscope in research very little, if at all; and when he offered an exposition of a new line of observation, he followed rather than led. At the same time, he possessed a

comprehensive idea of the range of morbid anatomy and the science which must spring out of it. He regretted much that the knowledge of morbid structure does not lead with certainty to the knowledge of morbid action, although the one be the effect of the other; yet surely, he contended, it lays the most solid foundations for prosecuting new inquiries with success, and in proportion as men become acquainted with the changes produced in the structure of parts from diseased actions, they are more likely to make some progress, though slowly, towards a knowledge of the actions themselves. In this there was prediction which is at last being fulfilled, although still slowly, very slowly, and with much diversion from the royal road towards positive science.

A second advantage which Baillie detected as likely to spring from a knowledge of morbid structure is, that by it practitioners will be able to distinguish between changes which may have some considerable resemblance to each other, and which have been generally confounded, an advance that must lead ultimately to a more attentive observation of symptoms while morbid actions are taking place, and be the means of distinguishing diseases with greater accuracy. Another advantage arising from the same line of study was, he thought, that men will be better fitted to detect diseased alterations in the organisations of parts which are little or not at all known. "This will lay the foundation of inquiry into diseases themselves, so that there will be added a knowledge of the pathology of the body, and perhaps also of remedies." So he wrote.

But the most important advantage from the study of morbid anatomy put forth by Baillie was that, from observing attentively morbid structure, theories taken up hastily about diseases will be occasionally corrected. "The human mind is prone," he argued, "to form opinions on

every subject which is presented to it; but, from a natural indolence, is frequently averse to inquire into circumstances which can alone form a sufficient ground for them. This is the most general cause of the false opinions which have not only pervaded medicine, but almost every other branch of knowledge. When, however, the mind shall be obliged to observe facts which cannot be reconciled with such opinions, it will be evident that the opinions are ill-founded, and they will be laid aside. We grant it does not always happen that men are induced to give up their opinions, or even to think them wrong, on observing facts which do not agree with them, but surely it is the best means of producing this effect; and, whatever change may be wrought on the individuals themselves, the world, which has fewer prejudices to combat, will be convinced."

I have said that Baillie in his work was a follower of Morgagni in respect to plan; and, on comparing the two for the special purpose of accuracy, I feel sure this view is generally correct. It is but fair, nevertheless, to say of him that he was not conscious on his own part of the fact, for, with simplicity of utterance, he declares that any works explaining morbid structure which he has seen are very different from his own: they either consist of cases containing an account of diseases and of dissections collected together in periodical publications, without any natural connection among each other; or they consist of very large collections of cases, arranged according to some order. In the large works the diseased structure has often been too generally described, and the descriptions of the principal diseased appearances have also been sometimes obscured by taking notice of smaller collateral circumstances, which had no connection with them, or the diseases from whence they arose. In his work he does "not give cases, but simply an account of the morbid changes of structure

which take place in the thoracic and abdominal viscera, in the organs of generation in both sexes, and in the brain." This is done according to a local arrangement, very much in the same manner as if we were describing natural structure, accompanied with occasional observations on morbid actions." His work contained chiefly an account of the morbid appearances which he himself had seen, and was intended to comprehend an account of the most common, as well as many of the very rare appearances of disease in the vital and more important parts of the human body. The nature of the work was progressive, and must have been so; since some appearances of disease with which all were then totally unacquainted, would, he knew, be observed in the future, and others which were little known of in his time would afterwards be known perfectly.

These were the principles he had in view in his earlier labours. In his later, as he tells us in the preface to the second edition of the work on "Morbid Anatomy," published on November 20th, 1797, he added some of the symptoms to each kind of morbid appearances, naming those symptoms "which are most constant and most strongly characteristic of the diseases to which they belong." The account of the symptoms was placed at the end of each chapter "that the anatomical part of the work might not be interrupted."

The passages above recorded afford a brief but fair statement, derived from the author's own words, of the plan adopted by Dr. Baillie in the construction of the essays on medical science which he has left to us. Their influence it is difficult now to estimate, for they have passed into the limbo of the forgotten. Yet, even I can remember when in the lecture-room they were quoted as authoritative, and when the reception of the "Morbid

Anatomy" as a prize-book was considered by the student who "took it" as an unusual stroke of good fortune. True, also, it is that to this day the student who may, by accident or from curiosity, take up the volume, and the papers which preceded it, will find matter of deep interest. He will, for the moment, be removed from current minuteness; he will find little about degenerations, nothing about pectous or adenoid changes, nothing about cell growths, nothing about microbes; but he will still find foundations, still find truths which have to be accepted as good truths, though started and stated in the most elementary form. Beyond all, he will discern the evolutionary character of what is sound in progressive medicine, and may, perchance, be led to see more clearly than he would, without the study, what is devolutionary and retrogressive.

Some Details of the Works.

The works themselves are divisible into two series—a preliminary series of miscellaneous essays on various subjects connected always with morbid anatomy, and a special work entitled "*The Morbid Anatomy of some of the most important parts of the Human Body.*" To the last work the Author wrote two prefaces, one to the first, the other to the second edition, and afterwards added to it the engravings for its elucidation made by Mr. Clift, the copper-plates of which were bequeathed to the Royal College of Physicians.

The Miscellaneous Papers. Recognition of Aphasia.

The miscellaneous papers and dissections include sixty-three short essays on various subjects relating to morbid anatomy in its bearings on practice. The most original of these is one entitled "*Case of Internal Hydrocephalus attended with some uncommon symptoms.*" This was the

case of a gentleman, aged fifty-six, who was seized with symptoms of compression of the brain on February 9th, 1805, and become completely paralytic on the *right* side on the 11th of the same month. *By this attack he lost the recollection of the words of his own language*, except a very few, which he pronounced with the greatest distinctness, and without exhibiting that thickness of pronunciation which is so common in paralytic patients. The words "Yes, yes, no, no, Mr. Reed, yesterday," were employed on all occasions, and with a great variety of tone, to express pleasure and displeasure, joy and sorrow; to explain the circumstances of his disorder, and to give directions about what he wanted. He did not seem to be aware that these words were not the proper ones to express his meaning, for he often betrayed impatience when he was not understood, and was not mortified at so often repeating the same words. He could pronounce other words distinctly, but he never did so, except to repeat some words first uttered in his presence. He died on January 6th, 1806, after passing through all the now well-known stages of paralysis with muscular rigidity, "the right arm resembling that of a Fakir in Hindostan." The symptoms in this case were so singular that Baillie was most anxious to examine the brain after death. He did so two hours after that event. The membranes and cerebral substance were perfectly natural in appearance. The blood vessels were not much loaded with blood, and there was no appearance of blood having been extravasated in any part of the substance of the brain. The lateral ventricles, however, were found to contain rather more than six ounces of water. There was no other diseased appearance, except that the left vertebral artery was enlarged in size, and that its coats had become opaque.

I give the report of this case at some length, because it

is the first recorded example in this country of the disease that has in our days been named *aphasia*.

This essay is immediately followed by another entitled "*Observations on a strong pulsation of the aorta in the epigastric region*," in which is described, with great precision, the curious phenomenon of epigastric palpitation, without aneurism, or other aortic lesion. The precise nature of the affection Baillie did not pretend to understand, and he did not connect it with the cœliac axis; but he was conversant with the fact that the phenomenon is met with most frequently in combination with dyspeptic symptoms. He was thus led to a more correct diagnosis of abdominal pulsation than had previously prevailed; and, although Wardrop declares that he was sometimes mistaken, he made without doubt an advance in detecting a condition of disease which had been unsuspected, had often misled practitioners, and had caused a comparatively harmless symptom to be mistaken for one almost certainly fatal in character.

The Work on Morbid Anatomy.

The temptation is strong to continue the description of the miscellaneous papers: but here I must conclude notice of them in order to refer, in the briefest terms, to the major work on morbid anatomy. This treatise, divided into twenty-four chapters, refers systematically to diseased appearances of the pericardium, the heart, the cavity of the thorax, the lungs, the thyroid gland, larynx, and parts contained in the posterior mediastinum, the cavity of the abdomen, the stomach, the intestines, the liver, the gall bladder and biliary duct, the spleen, the pancreas, the kidneys and renal capsule, the urinary bladder, the vesiculæ seminales, the prostate gland, the urethra, the testicles and spermatic cord, the uterus, the ovaria,

the Fallopian tubes, the vagina, the external parts of the female organs of generation, and the brain and its membranes.

It will be seen from this list how wide was the field of observation in the book on morbid anatomy, and a difficult task it would be to make any selection that would specially represent the industry of the author. The remarkable feature of the treatise is the even tenor of its course. Divided, it is almost commonplace; taken as a whole, it is almost genius, genius of honest observation and admirable industry. To the practitioners of its day it must have been veritable light thrown upon what had been the darkest places; but from its method and from its brevity it lacks the spontaneity of the miscellaneous essays. The observations on the chemistry of gall stones, on the nature of ovarian dropsies, and on the condition of the brain in hemiplegia in which the author infers, I believe for the first time in physic, that "the right side of the body derives its nervous influence from the left side of the brain, and the left side of the body its nervous influence from the right side of the brain," form the choicest of its pages.

THE CLOSE OF A BRILLIANT CAREER.

Up to the year 1823 Baillie continued in his brilliant course, working at the rate of sixteen hours a day amidst all the external bustle and wear of London life. From 6 a.m. till 8.30 a.m. he was occupied with letters; from 8.30 until 10.30, less a short interval for breakfast, he received patients at his own Grosvenor Street house; and afterwards, up to 6 p.m., he was out visiting patients, chiefly in consultation. The hours of 6 to 8 p.m. were consecrated to dinner, after which, often until a late hour, he returned to outdoor visiting work. At the same time he

disdained all artificialities, and not only refused the baronetcy, but after permitting himself, under much persuasion, to sit to Hoppner for a portrait, the autotype of which is supplied in the frontispiece of this Asclepiad, was so annoyed at seeing the engraving of it in the window of a print-shop that he bought up the copper-plate. He was most unassuming in manner and simple in his mode of life, caring nothing for show, and taking as little of pleasure as of repose.

It was but natural that a man of his rather feeble physique should wear out rapidly under the unceasing activity of mind and body to which he subjected himself. He became old whilst he was yet young, and began to feel exhaustion of overwork by the time he had reached his fiftieth year. He now treated himself to an autumnal vacation, and made a journey for the first time since he left it—about 1809—to Shots, his old Scottish home; but the change brought to him sorrow rather than relief. He bought an estate in Gloucestershire, on the Cotswold range, and would rusticate there, riding about on a small pony, seeking rest and finding none. From country quiet he soon felt desire to return to his home duties, tormented with unceasing *ennui* until he got back into work, and tiring in turn at that. He became, after a few seasons of this varied round, emaciated, and deficient in mental energy. In 1823 he was laid by from inflammation of the trachea, followed by recurrent fever and cough. He retreated, for a period, to Tunbridge Wells; but got no benefit, and acquired a dislike for food. From the Wells he returned to Gloucestershire, but gained no relief there, and after lingering for a time, suffering severely, he died on the 23rd day of September, 1823, in the sixty-second year of his age.

The news of the death of Dr. Baillie, when it reached

the metropolis, caused universal sorrow and demonstration of respect. The members of his own profession met in order to raise a subscription for a monument to his memory in Westminster Abbey. The Royal College of Physicians passed a resolution of sorrow and respect, and had it entered on their minutes. The politicians forgot their differences, and the Tories, who could hardly forgive a fashionable physician who declared himself a Whig, and would not give a Tory vote to a Tory nobleman for the county of Gloucester, joined with the rest of the community in sympathy.

But the highest honour and respect to Matthew Baillie fell from the lips of his most illustrious contemporary, Sir Humphry Davy, who, commenting on his memory from the chair of the Royal Society, concluded his panegyric with these words :—

" An honour to his profession in public life, he was most amiable in his most intimate social relations and domestic habits. No man was ever freer from any taint of vanity or affectation. He encouraged and admired every kind of talent, and rejoiced in the success of his contemporaries. He maintained amidst courts the simplicity and dignity of his character. His greatest ambition was to be considered an enlightened and honourable physician. His greatest pleasure appeared to be in promoting the happiness and welfare of others."

After such a tribute from such a master, who shall deny that medicine, in one of her sons, was nobly recognised?

VOLTAIC NARCOTISM AND ANODAL DIFFUSION.

DR. FREDERICK PETERSON, Physician to the New York Hospital for Nervous Diseases, has published more than once during the past two years on the subject of what he calls "*Anodal Diffusion*, or the ability of a galvanic current, flowing from the positive to the negative pole, to, seemingly, carry substances with it, even through a membrane or through the skin." Dr. Peterson has been good enough to send me his last essay on this topic, in which essay he most honourably pays me the compliment of having originated the theory and practice of anodal diffusion in a research on what I called *voltaic narcotism*, the details of which were published in the *Medical Times and Gazette*, of April 27th, in the year 1859. Previously to the receipt of Dr. Peterson's very kind favour, I had been written to by one of his countrymen for particulars of my old work on voltaic narcotism, a fact that caused me some surprise, for I had conceived that all interest in the matter had died out a generation agone. Now I see that a new hand has taken up the labour with so much candour, I understand the mystery.

In order to answer this correspondent I turned up the medical papers of 1858 and 1859 containing my researches at that time, in order to recall what they were

themselves, and what was said of them when they were newly born. And, as they now possess some new interest, I summarise them once again, fully conscious of the often repeated fault of nature or habit, of having set a new research on foot, and afterwards left it, in the weakness of its childhood, naked to its enemies, in order to follow some other original study which attracted me more, or which seemed to be of more immediate importance.*

General Anæsthesia from Electrical Shock.

Referring back to the views advanced in the years 1858-9, I find that my attention was first enlisted in the work that led to them as far back as 1853, in which year I was engaged in investigating, by experiment, the influence of electrical currents on the blood in living animal bodies. A dog was subjected to the electrical shock from a Leyden battery, the discharge being passed from the head to the loins. The effect of the shock was instantaneous, and the animal lay as if absolutely dead. Respiration entirely ceased, but the heart was still beating, and a little later the act of breathing recommenced. The animal was perfectly insensible to pain, but living: it was in a state of deep anæsthesia. In this state an operation was performed on it of laying bare and removing a section of the jugular vein on

* This was the case also with the study of resuscitation, septine, amyl nitrite, the caustic alcohols, oxygen, and other tempting pursuits; and as I begin to see, in the distance, the end of the vital vista, I feel the truth of the charge that I have not always made the best use of the trusts a too beneficent spirit confided to me. But, whatever may have been these sins of omission, they were unintentionally committed, and will I trust be forgiven. A man is what he is, not what he would wish to be.

one side. The operation was performed and completed without the slightest evidence of causing pain, the external wound was neatly closed with fine sutures, and recovery from the unconsciousness gradually took place. For a short time the respiration was irregular, but in a few minutes the animal rose slowly, and recovered without an untoward symptom.

This was probably the first instance on record in which an operation had been performed without pain by means of electricity. The new fact, accidentally brought out, suggested the idea of producing general anæsthesia by electric stun, but was not followed up because of the imminence of the danger. The experiment, nevertheless, gave origin to another conception, namely, that what could not be done to the body as a whole might possibly be done to a part of the body. Hence arose the question of the possible production of local anæsthesia by electricity.

Local Anæsthesia from Electrical Shock.

In following up this inquiry I passed electric shocks through localised parts of lower animals, and, obtaining no satisfactory results, I commenced to operate on myself. I charged twenty Leyden jars, and discharged them, either in combination or in rapid succession, through one of my fingers. The shocks were painful, and I observed that when many were given in succession the last were as painful as the first. The finger also was, afterwards, as sensitive to a prick from the lancet as it had been previously. I tried the effect of the continuous current for long periods with the same negative result.

I passed the electro-magnetic current through one finger for a long period, modifying the intensity of the shocks, sometimes submitting the part for periods of an hour to a gentle current, at other times increasing the

current till the pain caused by it was scarcely endurable. On January 3rd and on August the 8th of the year 1858, I kept a finger for two hours exposed to the current without the slightest effect in reducing sensibility; indeed, it seemed to me that the electric shock increased rather than lessened the sensibility. I made another experiment which endorsed that view. I placed two of my fingers in a freezing mixture, and held them in it until they were so benumbed by the cold that the prick of a needle gave no pain to either of them. In this insensible condition I removed them from the freezing bath, and, having dried them, passed through one of them the current from the electro-magnetic machine, while the other was left free. In the finger submitted to the current the sensibility came back much more quickly than in the free one, and so much more quickly as to give rise to the painful reaction called vulgarly "hot-ache." The current acted like warmth in this respect.

I performed another experiment, the results of which were very peculiar. I plunged the first and second fingers of my left hand into a freezing mixture, and held them there until they were quite insensible to pain. I then removed them from the mixture, and after well drying them, I placed one wire from the electro-magnetic battery round the second finger at a distance of three-quarters of an inch from the tip, and the other wire round the base of the finger. A gentle current was passed, and for a short time I was not conscious of the shocks; but suddenly the portion of the finger between the poles, from being surface white or blanched, became suffusedly red, and so extremely painful I was forced to remove the wires and re-apply cold in order to obtain relief. By this means the parts came back to their natural state, with one exception, on which a special note has to be made. The tip of the

second finger, which had been excluded from the electric current whilst all the parts below it were subject to the current, remained completely insensible for four hours, and did not regain its full sensibility for thirty-six hours after the experiment was finished.

These observations led me to the conclusion that the electric discharge, when it appears to produce a benumbing effect, as in the extraction of a tooth, acts only by creating a diversion of the mind, as in the schoolboy experiment of extracting a hair rapidly from the head at the same moment when a sharp blow with the flat hand is administered.

The conclusion I came to in regard to electrical anæsthesia in 1858 was that, applied generally, so as to strike out consciousness, the shock could be made to answer, at great risk to life; but that applied merely locally it was of no avail. A little later on, however, another experiment on myself slightly modified this view. My friend Dr. Julius Althaus made an experiment on my ulnar nerve. He passed a rapidly intermittent current along the ulnar of my left arm, from the point called commonly "the funny bone" to the base of the little finger. After a long application I did feel a slight benumbing, but not sufficient to answer for operation; and, curiously again, the numbness or sense of deadness was most distinct in the finger; that is to say, in a part beyond the course of the current and out of the circuit, exactly as had occurred after the experiment with cold and electric vibration. This fact I have not yet seen my way to account for or turn to account; but it is a fact to be remembered. It must mean a great deal.

Effects of the Continuous Current.

For many months following upon the publication of the essay noticed above, my experiments went on in order to

ascertain whether, by a strong continuous current carried through a local structure, any efficient degree of insensibility could be produced. The conclusion arrived at amounted to what was expressed in the following sentence: "The continuous current is incapable of producing any degree of anæsthesia in such a way as to be applicable as an adjunct to surgical art. I say an adjunct, because I do not wish to state that the chemical force of a continuous current might not be raised to such effect as to cause insensibility in a living tissue through which it should be passed. On the contrary, I think it could; but such insensibility would be bought at too great an expense. It would be induced only by actual decomposition of the tissues; it would be preceded by the infliction of unwarrantable pain; and it would prevent that reparative process which is essential to the success of surgical manipulation."

Effects of Pressure for Anæsthesia.

The autumn of 1859 was devoted to another line of experimental study bearing on local anæsthesia. In September of that year I tried systematically the benumbing effect of pressure on the nerves of my own body. I learned a method of placing one of my lower limbs in such a position that I could at pleasure, or, speaking more correctly, at pain, bring on the phenomenon known as "going to sleep;" but the result was *nil* in regard to the object I had in view, for I found that however thoroughly the limb was benumbed by the pressure, that although it might feel like a dead weight and its muscles might fail properly to obey the will, the external surface was sensitive to the slightest impression, and a needle point pushed into the skin created the same pain as it did in other parts of the same surface that were not under

pressure. The phenomena produced by the pressure were, in fact, practically the same as those observed from the long transmission of the intermittent electrical current: first, a tingling sensation, and then a sense of weight and "sleep," but with the cutaneous sensibility unimpaired.

SURFACE ABSORPTION AND VOLTAIC NARCOTISM.

I moved next to some experiments on simple absorption of different narcotic substances, in order to ascertain if, during surface absorption, there was any production of insensibility at the part where the narcotic was applied. I experimented with all the known series of narcotics, and in solution of aconite I found an excellent local anæsthetic, as good, I think, as that which has since been brought out in cocaine. I was not, however, content with the success that was obtained; I thought it was too limited for ordinary use: a conclusion a little too absolute, as we now see in the results which have followed the employment of cocaine for superficial operations.

At this stage the thought occurred to me that perhaps an electric current from the positive pole might aid in conveying a narcotic into the tissues during absorption, and so anæsthetise locally. Charged with this idea, I got my good friend Mr. George Gregson to submit himself to experiment on October 31st, 1858. I placed a sponge, dipped in a solution of morphine, on his arm, and covering the sponge with a copper plate connected with the positive pole of a voltaic battery, and fixing the negative pole to a moistened sponge placed under a copper plate a few inches lower down on the limb, I caused the current to pass, and produced in a quarter of an hour a condition I had never seen before; for on removing the plate connected with the positive pole, I found the part over which the narcotic had been applied pale in colour and quite

insensible to pain. Pricked with a needle in this part, Mr. Gregson experienced no sensation whatever. The experiment was a decided encouragement to further research.

After a good many more experiments, I ventured to apply this local anæsthetic process to an operation on the human subject, in February 1859, in order to enable Dr. Halford to transfix and tie, painlessly, a nævus, the size of a shilling-piece, on the right shoulder of a child ten weeks old. In this case the negative pole was made to surround the nævus by means of a ring of copper cooled with moistened sponge, whilst the nævus itself was covered with a disc of copper, having beneath it a thin layer of sponge which just covered it, and in connection with the positive pole of the battery. The sponge, over the nævus, first moistened with water, was charged with a narcotic solution composed of five minims of tincture of aconite and five of chloroform. The continuous current from the battery was now passed for a quarter of an hour, and on the sponge being removed the part beneath was found to be white. We reapplied the sponge, after adding another charge of the narcotic solution, of the same strength as before, and again passed the current for the same period. I believed the part now to be ready for operation, but Dr. Halford, anxious to be quite sure, urged two more applications of the narcotic, after which he proceeded to operate. He transfixed the nævus through its base with two long needles, and ligatured the mass with all the firmness he could; but neither the transfixion nor the ligature caused the expression of the slightest pain. The infant fell asleep directly after the operation was completed, and recovery was as simple and good as could have been desired.

On the next day I applied the process for the extrac-

tion of a tooth, the late Mr. Kimpton, of Princes Street, Hanover Square, being the operator. The tooth, the last upper left molar, was very firm in its place. The plate from the negative pole was placed externally, immediately below the lobe of the left ear; a cavity in the tooth was filled with a dossil of lint, charged with five minims of tincture of aconite and five of chloroform, and into this the point of a conductor from the positive side of the voltaic battery was attached. The current was now set up, and after being applied for five minutes, we found that a sense of numbness had commenced, and had extended, superiorly and laterally, from the tooth as a centre. A new dossil of lint was inserted in the cavity of the tooth, with a new charge of the narcotic solution, and the current being re-established, it was found, in from two to three minutes, that the sensation of shock was not felt when contact was intentionally broken. After seven minutes the poles were removed, and Mr. Kimpton extracted the tooth without the patient expressing the least sensation. The patient explained that he could hear the breakage between the tooth and its socket, but without the slightest sense of pain, and indeed without common sensation.

In a succeeding trial I punctured subcutaneously a bursa on the wrist of a girl. The plate connected with the negative pole of the battery was placed on the lower part of the fore-arm, while the pole from the positive side, attached, as in previous cases, to a copper disc covering a piece of sponge, and carrying also five minims of tincture of aconite and five of chloroform, was placed over the bursa. The current was made to pass, and the application was three times repeated during sixteen minutes. The skin under the copper plate again became white, and at the time of the operation the insensibility

was so complete that the patient, whose head was averted, had no consciousness that anything was being done. Sixteen hours afterwards the centre of the part on which the narcotic had been applied was still insensible to some degree, but recovery was excellent. As a test experiment on this patient, which she was quite willing to permit, I applied the same kind of narcotic solution to another part of the arm under the copper plate without the voltaic current. At the end of the half-hour the skin beneath the copper plate was slightly reddened, but there was no insensibility.

During the succeeding four months of the year 1859, I continued these researches with varying results. I made the conjunctiva perfectly insensible to the knife, so that the recti muscles could be divided painlessly; I applied the process with much but not perfect success, whilst the late Mr. Hancock operated successfully on a patient suffering from inguinal hernia; and I used it, with entire success, whilst the late Dr. Vesalius Pettigrew removed a tumour, the size of a large orange, from the shoulder of a woman twenty-five years of age. This last operation was really remarkable for its success. I placed over the tumour a hollow copper plate, with lint beneath moistened with the chloroform and aconite solution, and connected with the positive pole of the voltaic battery. The conductor of the negative pole, armed at its end with moistened sponge on a copper plate, was placed on the flat surface of the shoulder, below the tumour. As it was necessary, for the removal of the tumour, that ten inches of the skin should be divided and deep dissection should follow, I continued the application with the current for a full hour; after which, the plates being removed, the operation was performed. The patient was entirely unconscious of the

entrance of the knife and of its course; but as the tumour was being dissected out she gave expression of some indefinable sensation. After the removal of the tumour and the closure of the wound by the sutures, the patient, when she was calm and collected, explained that she had no knowledge of having been cut by the knife, nor of the passage of the needles on the insertion of the sutures; but that at one point during the operation she experienced what she called a "wrench," as if something were being removed by force.

From other and later effects of local anæsthesia I have never seen any better result than in the case above cited. More thorough local insensibility could not have been obtained by any process, and there was along with it this advantage, that the surgeon was not for a moment embarrassed by hardening or other alteration of tissue. But I again confess, as I confessed originally, that every operation under the method named was not a similar success; on the contrary, there were some remarkable entire failures, whilst there was no real half failure, a fact which much surprised me, and which I have never been able to explain to myself, up to the present hour, on any other supposition than that from some accident on my part I sometimes did the thing that was completely right, and sometimes, by some inexplicable oversight, I failed to do the right thing altogether. Dr. Halford brought me a patient with a whitlow, for immediate operation. I applied the local plan for two hours, but when the operation was performed the deep cutting was as acutely felt as it would have been expected if no narcotic had been used. Dr. Russell Reynolds brought to me a man in whom, for some nervous disease, he wished to produce a deep insensibility at one part of the arm; I applied the process, frequently reapplying the narcotic

solution, for a period of three hours; but, to my surprise, not the slightest effect, either locally or generally, was produced. On the whole, the conclusion I arrived at was, that although it might be accepted as a fact that, by voltaic narcotism, such a degree of insensibility could be induced, in certain instances, as to enable the surgeon to operate without the infliction of pain, the process could not be depended upon as generally applicable, whilst occasionally it failed altogether.

Results of Theory.

I observe that Dr. Peterson expresses that I gave up the practice as well as the principle of voltaic narcotism owing to "the storm of opposition aroused over Europe" to the procedure. In this he is a little in error. It is true that one of my able contemporaries, a man of genius charged with an ungovernable fire, did, *more suo*, go into a temper on the subject; but I let his criticism burn out, and I never knew that he did the least harm to the research, no more, in fact, than I did myself in telling everything, unfavourable as well as favourable, in respect to it. There was no storm; on the contrary, I never did anything that brought me more distinguished sympathy. Professor Eckhart, Mr. Nunnerley, Dr. Stokes, Dr. Russell Reynolds, Mr. Fergusson, Mr. Hancock, Dr. Halford, Mr. Spencer Wells, Dr. Watson, Professor Siliman, Dr. Tholozan, and many more, expressed, in warmest terms, their interest in the project, and their hopes for future success; whilst Dr. John Davy looked upon it as, indirectly, one of the happiest evidences of his illustrious brother, Sir Humphry's, wisest forecasts.

The real reasons why I did not pursue the inquiry were: (1) That the effects were uncertain; (2) That the time required for the reduction of sensibility was too long;

(3) That during the application there was sometimes a certain degree of pain from shocks on making and breaking contact; (4) That after the application, when it was prolonged, there was occasionally irritation and soreness, amounting in one or two instances to superficial vesication, which was decidedly disadvantageous.

These were the objections which prevented me from sustaining the practice of voltaic narcotism; and they were, in my opinion, amply sufficient. If the anæsthesia had been more certain than it was, I believe that the time required for its production would alone have led me to abandon the process; but the combination of difficulties left me no alternative, and so I gave the plan up in order to seek something better.

Ultimately, I found what did answer much better in ether spray, but I have never lost interest in the voltaic method as an experimental and academical, if not a practical method, the point requiring most attention being whether the anæsthetic results were due, when they were produced, to *simple absorption* of the narcotic, or to *absorption aided by the current.* In plain words, did the current carry the narcotic with it in its course? My chief opponent, the late Dr. Augustus Waller, came to the conclusion that the current had nothing to do with the result, and that whatever good came from the application was due to ordinary absorption, and that alone.

My old and valued friend, Mr. Nunnerley of Leeds, wrote to me recalling my attention to his experiments in 1849, in which he showed that the mere local application of some narcotic solutions to a part may produce a local anæsthetising effect. I looked, thereupon, into the question closely, to see if I could have been deceived, and had simply added, in the voltaic current, an influence which

was not necessary. To solve the doubt that had in this way arisen, I repeated many of the experiments, and found two unexpected facts. I found that on the inferior animals some of the operations I had trusted to, for evidence of painlessness, could be performed without any indication of pain when no anæsthetic process was resorted to. I found, further, that the local application of the narcotic solutions on the surface of the human body was often so effective in reducing sensibility, that there was a chance of deception in regard to the value of the current; and, finally, I came to the conclusion that the current itself had no anæsthetic effect, but had the power of modifying the capillary circulation in the structures through which it was transmitted, and that the absorption process might perhaps be rendered more rapid and determinate by its action.

I was not, I expect, very far from the truth in this last view of the action of the current and of its power in quickening absorption, and transmission by diffusion of the medicinal substance into the tissues. To a similar conclusion Dr. Peterson has himself come, for I find in one of his latest essays (*Times and Register*, March 21st, 1891, p. 254) the following passage :—

"There can be no doubt that the effects of the galvanic current upon nutrition are in part due to the cataphoretic transfer of molecules of protoplasm and liquid, from one cell to another, or from a cell to a capillary vessel, in the path of an anodal stream; and, since the diffusion takes place more readily and more quickly in direct proportion us to the strength of the current, it behoves us to employ as many milliampères as feasible in our galvanisation of the atrophied extremities of poliomyelitis, chronic neuritis, and peripheral nerve trauma."

There still remains the crucial point whether more than

ordinary absorption takes place; whether, in short, the current really leads to diffusion through deep structure from the surface into the blood, so that substances can be introduced and eliminated by the excretions by virtue of an electric current. The question is beset with difficulties, and for this reason, that absorption of many soluble substances will take place by simple diffusion without the interference of the current. In considering, therefore, the added influence of the current, we must trace out the evidence by three lines: (*a*) By subjective effect; (*b*) By time of effect; (*c*) By the direction of effect from subjective tests on the human subject made with narcotic agents combined with the current. I must express that there does seem a *primâ facie* case in support of the idea that the current favours the production of anæsthesia from narcotics of a certain class. I am forced to admit that in the same person on whom I have produced good local anæsthesia of the skin by narcotic solution with the current, I have failed to produce a similar result with the narcotic without the current. This looks like specific anodal diffusion, and I can explain the phenomena on no other ground.

Dr. Peterson, if I read him correctly, is certain that different degrees of electrical intensity produce different results; and if that be so, we must admit the specific effect. My own observations ran in the same direction, and I can feel no reasonable doubt, that when the current was carried up to the border of endurance there was a quicker anæsthesia than when it was less intense; and, indeed, I used the current in this way as a test of action, by recognising that when a painful current was established, the fact that it ceased to be felt on making and breaking contact, was the proof of the narcotic effect. A feeble current might be maintained indefinitely without

effect, while a strong current would, in a short time, obliterate the pain it at first excited. This seemed to me to establish three facts: (1) That the narcotic alone was insufficient; (2) That the current was effective according to strength; (3) That as that current, by itself, produced no insensibility, the production of the insensibility, when the combination of the narcotic and the current was made, showed that such combination was a specific reason for the phenomenon. I am not inclined to move from this conclusion, but I do not say that the evidence is complete, because it may be argued that ordinary diffusion was the cause of the insensibility.

On the point, the direction of diffusion, I am much embarrassed. If diffusion take the direction of the current through the tissues lying in the course of the current, why should there be diversion of the narcotic substance into the blood and appearance of it in the urinary and other secretions? Assuming nothing more than absorption to be at work, the osmosis into the blood is easily enough explained; it is as if the substance had been swallowed, or injected into the bowel or under the skin. But I cannot understand what is called "electrical endosmosis," as applied to diffusion of soluble or volatile substances through the body by an electrical current. An experiment performed by Dr. Walling, in which he placed a solution of potassium iodide on the negative pole, and, laying over that solution a layer of coagulated egg albumen, caused the iodine of the salt to pass through the albumen, and appear at the positive pole when he made contact, is quite understandable; for here he decomposed the iodide salt, and, setting the iodine free, ensured its diffusion through the albumen, as readily as if he had exposed the albumen to vapour of iodine; but that does not explain diffusion through the living tissues from one pole to

another, with the blood as a diverting channel and itself an absorbing endosmotic fluid.

I confess, also, that I do not see how the decomposition of chloroform, and of the complex alkaloids by the electric current accounts for diffusion by it. If they were so broken up, they would no longer display, as they do, their special physiological action. Neither, on the other hand, do I see how a current can carry such bodies unbroken in its own direction, or diffuse them through living tissues. It could only do so in a mechanical manner, as if it divided them infinitesimally, and yet kept them within the sphere of its influence. Altogether, I must still infer that the action is local in all cases where the medicinal substance is a stable product. With unstable substances which, like potassium iodide, decompose under the current and come under the rule of electrolysis, the circumstances are different; but we are now not dealing with electrolytical phenomena: we are much nearer to vital processes if we are near anything at all.

Results of Practice.

Moving from theory of action to action in practice, there is a satisfaction in knowing that, though the effect be merely local of local, it is attended with good results in a rather large class of painful diseases. As a method of local anæsthesia in surgical procedures, I could not recommend it against ether spray, or the local application of cocaine, or the carbolic ethereal spray which I have used so effectively in selected cases. But for topical use in neuralgias, and for long-continued superficial pains; for topical application in various local lesions of the mucous or cutaneous surfaces; and, for the dispersion of some growths and tumours, I conceive that the method has a wide range of usefulness. It is good

to the extent that it may locally favour absorption in circumscribed areas.

From the report of the Philadelphian Society, before which Dr. Peterson read his last communication, I find that there was some difference of opinion in respect to the mode of using the electrical method in combination with medicinal substances. Dr. Peterson employed the current, in all cases, for the real or assumed diffusion from the anode, and for this reason called the process one of anodal diffusion; but Dr. Walling, who took part in the debate, desired to present a different view as to the action of the poles of a galvanic series upon some of the substances applied for diffusion. It was his practice and teaching, that the chemistry of the poles and of the substance used should bear the proper relation to each other. For instance; with such substances as the hydrochlorate of cocaine, the morphine salts, quinine salts, aconita, hyoscyamine, and, in fact, all the alkaloids, when he wishes to introduce their base into the tissues, he uses the anode; but with drugs in which, not the base, but the substance that takes the place of an acid is desired, he uses the cathode. Thus, for the iodides and the bromides he would use the cathode, as in the experiment where iodine derived from potassium iodide, was deposited on the anodal extremity after it had passed through coagulated egg albumen. To this Dr. Peterson demurs, and expresses the opinion that diffusion, or, as he designates the process, "cataphoresis," is an independent process, and does not depend on electrolysis. I should speak on the same side. All my experiments were, in his language, "anodal," and if the hypothesis of diffusion of medicinal substances by the current be true, I hold with him that electrolysis has nothing to do with the matter, but that all the phenomena depend upon the

course of the current, and are due to some direction given to osmosis by electrical action, in a special line of direction, determined by the current from the positive to the negative pole, the tissues being the intermediate conductors. At the same time I freely admit the difficulty of accounting for such conduction through changing structures with the blood in circulation through them. Diffusion in the opposite direction in connection with electrolytical disturbance of the substance employed I conceive, at this moment, to be impossible.

I put forward these difficulties with all diffidence, and, as the lawyers say, without prejudice. I certainly do not wish to imitate Saturn, by swallowing up one of my own children; but above all things I would not, without good absolute evidence of its truth, support a method of treatment because it first emanated from me. I know, if the method be sound, that its new advocate, Dr. Peterson, and his friends have not over-estimated its value; for, in fact, it opens up quite a new field of therapeutical procedure, one that will enable the practitioner to treat disease in the most active manner, without interfering with the digestive canal by drugs or by any foreign assimilation. The importance of the process cannot be over-estimated, if it be a true and sound process; but for this very reason it must be established beyond dispute before it is enforced. For my part, I will return as a fellow-student to the old studies, and in a few months will report what I have gathered from new research, and if at the same time others join in, all the better. In the research nothing specially complicated is required. Dr. Peterson and other inquirers have invented rather complicated electrodes; but, after all, there is no need to move from the simple plan which I adopted at first, of using terminals of copper discs, with a layer of sponge or lint holding the medicinal

substance at the anode, with a simple water sponge at the opposite pole. I should suggest that, for the present, we should put electrolysis altogether aside, and confine our observations to the use of substances that are stable in composition. We shall in this manner secure a fixed and common principle of action, and shall be able to determine whether the electric current, *plus* the natural power of absorption, aids or quickens absorption, the original crux on which the fate of the process entirely rests.

I cannot close this paper without expressing the sincere pleasure I feel at seeing these early experimental labours —labours forgotten here, and forgotten indeed by myself, until revived and recast—so well remembered and nurtured in America. These and similar remembrances often tempt me to visit my brethren there, in order to learn from them in their own fields of labour, and to tell them, personally, how deep is the obligation for their frequent and generous recognitions.

COTEMPORARY PRACTICE AND LITERATURE.

"*Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together.*"

*THE INFLUENZA EPIDEMIC.**

[BY GEORGE BUCHANAN, M.D., AND H. FRANKLIN PARSONS, M.D.]

THE Report on the Influenza Epidemic of 1889-90, drawn up by Dr. Parsons, and issued by the Local Government Board, is historical, and will remain a work of reference in the medical libraries of the future. Dr. Parsons has had at his command sources of information which no one else could have commanded, and he has used his opportunities industriously and ably. His volume, which contains three hundred and twenty-four closely-printed pages, opens with an introduction by Dr. Buchanan (the Medical Officer in Chief of the Department), in the usual form of a letter addressed to the President of the Local Government Board, the Right Honourable C. T. Ritchie, M.P., and Dr. Parsons' work is divided into thirteen chapters, dealing with (1) The previous history of the epidemic; (2) The history of the epidemic of 1889-90; (3) Clinical history; (4) Etiology;

* *Report on the Influenza Epidemic*, 1889-90. By Dr. Parsons. With an Introduction by the Medical Officer of the Privy Council. Official Report, 1891.

(5) Extent and fatality; (6) Prophylaxis; (7) Collective inquiries through Medical Officers of Health; (8) The Influenza Epidemic of 1889-90, as it affected the Public Services; (9) Behaviour of the Influenza Epidemic in Public Institutions; (10) Notes on certain inquiries by Dr. Bruce Low; (11) Local reports on the Influenza Epidemic; (12) Notes on the Influenza Epidemic by unofficial medical observers; (13) Postscript on the recurrence of epidemic Influenza in 1891, with notes by Dr. Bruce Low.

The introductory letter by Dr. Buchanan is short, consisting of rather less than four pages, in bolder type than in the remaining portion of the volume. It consists of a brief preliminary, and of notes on the origin of the disease; the manner of extension; the specialities of the extension; and the need for further information. The most distinctive feature of it is the uncompromising acceptance of the theory that, "in its epidemic form, influenza is an eminently infectious complaint, communicable in the ordinary personal relations of one with another." "It appears to me," he adds, "that there can henceforth be no doubt about the fact." About a fact there ought to be no doubt, and Dr. Parsons has, in every sense, done his best to arrive at the fact; but whether he has arrived at it, is still the question.

Dr. Parsons places before us two types of the epidemic passing under the one name—the minor epidemics of 1840-47, 1843-4, 1851, 1855, 1858, and the major epidemics which stand out in the years 1834, 1837, 1847-8, 1890, and 1891; and, he distinguishes these by writing the minor influenzas with a small *i*, the major with a capital *I*. By this division he has made a good mark of classification, and has brought us back

to the basis of the old nosology, in which the spreading catarrh was separated from a milder epidemic similar in character. He might have moved another step; he might have included common epidemic catarrh as a yet milder representative of a common malady, but he was probably deterred by the circumstance that the common epidemic catarrh leaves directly no victims; so that, as a cause of death, it is not on the record.

A second observation carries with it an important fact, namely, that in connection with the epidemic periods, there is not only an increase of mortality both from *I*nfluenza and from *i*nfluenza, but a concurrent increase from chest diseases generally. Also, "to a less extent, there are indications of a similar addition to the deaths from diseases of the organs of circulation." This is all very true, and it opens up the question whether the disease be something directly affecting the healthiest bodies, or whether it merely picks off those who, owing to some pre-existing disease of the respiratory and circulatory systems, rank amongst the *morituri*, the ready to die.

In my own experience, both in the latest epidemics and in that of 1847, as well as in the intervening minor epidemics, the evidence is in favour of the view that it is the *morituri* who die. The same would die under any other cause affecting the organs of respiration and circulation; such a cause, for example, as chloroform administered by inhalation. But, after all, this merely links Influenza and *i*nfluenza with common catarrh; for in seasons of catarrh these same *morituri* die from bronchitis, or pneumonia, or cardiac collapse, consequent upon "taking cold." In the absence of influenza, I never knew a fatal case of bronchitis or pneumonia that did not spring from "taking cold." On the other hand,

I never have known a death from Influenza or *in*fluenza, that was uncomplicated by bronchitis or pneumonia; and if all deaths were assigned during epidemic seasons to the organic tissues of the lungs or heart, the return would still be correct. When the return is Influenza simply, it means a rapid development of disease affecting the respiration or the circulation.

A third observation is the universality of the disease in its epidemic form throughout the world. With infinite care, Dr. Parsons has followed the epidemic of 1890 from one part of the globe to the other, until all is encompassed, the result of his observation being "that the disease has prevailed independently of season, climate, and weather. As in former epidemics, it has prevailed nearly at the same time; at places in the north and south hemispheres, *i.e.*, in opposite seasons of the year. It has prevailed in the cold of Russia and the heat of India; in the moist climate of the British Isles and the dry air of Egypt. In Spain, its advent was preceded by a month of dry, frosty weather, and at New York by the mildest and moistest season on record."

A fourth observation is the clinical history of the disease in which, in addition to the epitome of the well-known symptoms, it is shown that the disease attacks a large percentage, in some instances 75 per cent., of a community: that no age is exempt from it; that sex presents little variation, although males seem to be more generally affected; that one attack is not preventive of another; that relapses are common; that the morbid anatomy is that of the sequelæ, so that there is no true morbid anatomy of the affection pure and simple; and that the bacteriology of the disease cannot yet be said to be settled.

A fifth observation bears on the etiology of the disease,

and on this subject a most interesting and valuable chapter is compiled. Here is retold the old story of the inhabitants of St. Kilda who live in such pure air that the intrusion of a stranger, however healthy he deems himself, gives them the "stranger's cold." But the most laboured part of this section is the exclusive part in which all hypotheses as to causation are set aside in favour of the idea that the disease spreads by infection, spreads from a living organism passing from one person to another, and after a short incubation giving rise to the specific phenomena.

It was almost inevitable, in the present day, when the vital or animalcular hypothesis is again the rage of research, to escape this conclusion. Yet it is really as devoid of demonstration as the rest. No specific microbe has been detected; no reason is supplied why, in a household, one person should be stricken and all the others left free; and not a shadow of an explanation is offered why a living organism should exist, flourish, and propagate under the most opposite conditions of heat and cold, dryness and moisture, season and elevation; and, under every vicissitude, produce precisely the same phenomena of disease.

Lastly, there is no proof given that there is any longer period of incubation than is common to ordinary catarrh, which means that incubation as a preliminary condition and distinctive process is not proven. Dr. Parsons admits many of these difficulties, for which he deserves as hearty thanks as for the remarkable industry and clearness of exposition exhibited from the beginning to the end of his most instructive report.

DISCUSSION ON ANÆSTHETICS.

[EDITED BY J. WALKER DOWNIE, M.D.]

THE book published under the above title, and edited by Dr. Downie, is a volume of one hundred and thirty-six pages reprinted from the *Glasgow Medical Journal*. The discussions recorded were carried on in the Medico-Chirurgical Society of Glasgow, under the presidency of Dr. William McEwen. The first meeting was held on October 17th, 1890, when the opening address was delivered by the President. In this address the outline of the subjects to be considered was described, and it was proposed that as far as possible the observations of the various speakers should be arranged under the three following heads :—

1. The relative value of anæsthetics.

2. Their mode of administration, including the examination and preparation of the patient.

3. Fatal issues; how they arise, and how to meet them.

To these heads the President added a rider in the form of a question :—"Is it possible in the emergencies of practice to have two registered medical practitioners always present when an anæsthetic is administered? Were a law to that effect established would it increase human suffering or endanger life?"

Under the lines thus set out the discussions went on through three meetings, the most distinguished physicians and surgeons of Glasgow taking active part. At the first meeting Drs. Coats and Henderson, with Dr. Hartley of Leeds, expounded their views; Dr. Coats on "The Action of Anæsthetics on the Respiration

and Cardiac Action, and on the Risks Incident to this Action;" Dr. Henderson, "On the Relative Value of the Various Anæsthetics;" Dr. Hartley, "On Ether as an Anæsthetic." At the second meeting, held on October 24th, 1890, Dr. Kirk, Sir George Macleod, Dr. William L. Reid, Dr. Woodburn, Dr. W. J. Fleming, Dr. Beatson, Dr. G. A. Turner, Dr. M. Cameron, and Dr. Renton, added their quota, dealing, in turn, with "Chloroform Syncope;" "Anæsthetics in relation to Midwifery;" "Anæsthetics in Dental Practice;" "Nitrous Oxide, Cocaine, and other Anæsthetics;" "Causes of Death under Chloroform;" and "On Various Anæsthetics with Special Reference to Methylene." At the third meeting held on October 31st, 1890, Dr. George Buchanan, Dr. David Newman, Dr. Hector Cameron, Dr. James Dunlop, Mr. H. E. Clark, Dr. Alexander Robertson, Dr. Ness, Dr. Walter Downie, Dr. Samuel Sloan, Dr. McConville, Dr. N. Carmichael, Dr. McGregor Robertson, the President, Dr. Stirton, Dr. Lapraik, put forward their views, treating, in order, the following topics: "The Clinical Aspects of Anæsthetics;" "Experimental Observations and Influences relating to the Physiological Action of an Overdose of an Anæsthetic;" "Influence of the Respiratory Movements on the Systemic and Pulmonary Circulations—Changes in the Lung and Cardiac Impulses—Indications of Danger in Chloroform Narcosis;" "The Administrations and Dangers of Anæsthetics;" "Special Remarks on Ether;" "Anæsthetics in Disease of the Heart;" "Anæsthetics in the Children's Hospital;" "Anæsthetics in Practice;" "Anæsthetics in Obstetric Practice." The Presidential summary of the questions discussed, and of the views advanced by the speakers brings the report to a close. A careful study of this

report reveals that not one section of it is wanting in good and sound thought and expression, and that some sections are of the greatest value. In fact, the debate marks a stage in the controversy on general anæsthesia, and the conclusion of the learned President is so terse, I cannot omit giving it word for word.

"In conclusion, it may be that the future will disclose a safer anæsthetic than any which we yet possess. However right and commendable it is for experimenters to endeavour to procure new and better anæsthetics, surgeons ought to hesitate before they try upon their patients every new and untried agent, however much it may be lauded from animal experiment. Let the student study the physiological action of chloroform, learn that it is an agent which places the functions of the cerebro-spinal axis in abeyance, but does so seriatim in a definite order, and which may, if pushed far enough, suspend them all. Let him be fully alive to its dangers, and how to counteract them and let him recognise the safety zone of chloroform narcosis, and he will find it an anæsthetic, powerful, it is true, but controllable, and though not infallible, yet a most valuable and useful servant. Meanwhile, chloroform is one of the safest and best anæsthetics, though it requires for its proper administration, knowledge, skill and assiduous attention."

If this be the conclusion to which Dr. McEwen was led from the whole of the evidence which came before him as presiding judge of the whole, then the essence of the argument is for chloroform and nothing but chloroform. Then, also, chloroform is dangerous or not dangerous, according to the knowledge and skill that

is shown in the administration of it, and if we could master its method of action we could divest it of danger. How many thousands will join with me in saying, Would that this estimate were true to the letter! I, for one, dare not endorse it. From the day when chloroform came into action as an anæsthetic until now I have been a student of its action; and, unfortunately, from a practical point of view, I know no more at this moment, of what Dr. McEwen calls the "safety zone of chloroform narcosis," than I did forty years ago. This same experience, too, crops up constantly in the report, and has been the experience of the best men and observers I have ever known. Snow, who was an enthusiast for chloroform, could never define the range of its danger, could never explain why in one person it caused vomiting or collapse, and in another person no symptom of the kind. Fergusson, with whom I often spoke on the subject, was candour itself in his expressions as to the uncertainty of the action of chloroform. But I must not wait longer at this moment. There is matter enough in the report to call for further special notice in a future number.

CHICAGO'S DARK PLACES.

LET those who would like to know the social foundations of one of the latest built cities of the nineteenth century, read a very remarkable report, prepared for the "*Craig Press*" by a "Chief Commissioner," who, with a staff of assistants, carried out all the inquiries on which the report is founded. As a city suddenly rising into existence, Chicago is one of the wonders of the earth.

There is a physician still living there, the distinguished Dr. Nathan Davis, who remembers the day when this minor Babylon consisted of a few small houses; now it is so large and influential it is to be the centre of the World's Fair of 1893. It might be supposed that a city springing out of our modern civilisation in this manner, a city also that has once been nearly destroyed by fire and has risen up again from its ashes, would be a model city, a social paradise. How far the expectation is realised this volume, on Chicago's Dark Places, tells us in truly a dark picture. In this city poverty, degradation, and destitution are shown as sitting side by side with the wealth and luxury that are so prominent, in appalling proportions. Three thousand six hundred saloons, many of them perfect dens of every possible iniquity, are, it is affirmed, gathered together; "immoral Dives" is met in the most open manner; obscene pictures, books, and advertisements abound; the social evil is gigantic in its extent; there are more houses of prostitution, open night and day, than there are churches; there are degraded centres of the worst kind called "massage parlours, where prostitution in the gauziest of disguises is rampant." These, in brief, are a few of the revelations of "the *Craig Press* commission," but there is a chapter on the making of criminals which declares that "in the playgrounds and surroundings of the public school the children of Chicago are exposed to a training that is far more dangerous in its viciousness than words can adequately express." The volume ends with a chapter on "Suggestive Remedies" which promise well, if, even in less drastic form than is proposed, they could be carried out.

OPERÆ PRETIUM EST.

SINCE the issue of Dr. Parsons' Report on Influenza, Dr. Richard Sisley has published a treatise of 150 pages (Messrs. Longmans), entitled "*Epidemic Influenza, Notes on its Origin and its Method of Spread.*" The author makes his entrance into the field of medical literature in this work, and a very fine entrance it is without question. In many points he goes over the same ground as Dr. Parsons, and he comes to the same conclusion as to the spread of the disease by contagion.

Dr. Thomas Glover Lyon also publishes a short essay on Influenza (Baillière, Tindall, & Cox), but dealing with the affection in its "*clinico-pathological aspect.*" He, like others of us, looks on influenza as a disease attacking chiefly the central nervous system, and holds that symptoms referred to other parts, consisting "of loss of vascular, functional, and sensory tone," are due to a damaged state of the tonic centres in the central nervous system. His specific treatment is "rest of body and mind;" the "good dulness" of sleep.

"PRICHARD AND SYMONDS, *in especial relation to Mental Science, with chapters on Moral Insanity*," by Dr. Hack Tuke, is a small but handsome volume (published with their usual excellent taste by Messrs. J. & A. Churchill). Dr. Tuke is always good on biography, faithful, appreciative, sympathetic; and, in this volume, he is as fortunate in his subjects as in his delineations. Two such men as Prichard and Addington Symonds are not often met with even in a long lifetime, and the manner in which they are here represented is indeed delightful. We live with them, for a moment, again, and learn from them once more.

"GLIMPSES OF NATURE," by Dr. Andrew Wilson (published by Chatto & Windus), is a holiday book of science by one who is not only a facile writer, but who treats of natural things as a student living with them and never tired of his companions. Let all who want a seaside book read *Glimpses of Nature*, and especially "*Starfishes.*"

"A BIBLIOGRAPHICAL CATALOGUE OF MACMILLAN & Co.'s PUBLICATIONS FROM 1843 TO 1889," forms a handsome volume, and indicates how, from very small beginnings, one of the largest and one of the highest-class publishing businesses of this country has been established in rather less than half a century. Capital portraits of the founders of the business—one of Daniel, the other of Alexander, Macmillan (the latter still, happily, with us)—grace the pages of this curious and valuable historical work.

"THE INDEX AND HANDBOOK OF THE PERIODICALS OF THE WORLD," recently brought out by Mr. W. T. Stead, in connection with the *Review of Reviews*, is a marvel of industry and a perfect treasury of useful information. Such a compact, cheap, and informing chapter never, surely, saw the light before.

"A HOME HANDBOOK OF HYGIENE AND MEDICINE," by Dr. Kellogg, is a library of Hygiene for the household, and contains a large number of good illustrations.

"LEWIS'S MEDICAL VOCABULARY" (Lewis, Gower Street), has reached a second edition. It is a very convenient pocket reference book, carefully compiled and strongly bound.

POST SCRIPTA.

SYLLABUS OF LECTURES BY W. HEWSON, F.R.S.

As a postscript to the life of Hewson in last ASCLEPIAD, I publish the following syllabus, for which I am indebted to Dr. Addinell Hewson, of Philadelphia. The syllabus is valuable as indicating studies of a hundred years ago. Have we improved on a hundred years?

ADVERTISEMENT.

"Mr. Hewson begs leave to present this first part of his Syllabus to those who do him the honour of attending his lectures. The second part, containing the Lectures on Surgery and the Gravid Uterus, will be ready to be delivered by the time that the Lectures on this first part are finished. He likewise begs leave to observe that, although he proposes adhering as closely as possible to the order laid down in the Syllabus, yet he may occasionally find it necessary to deviate a little sometimes from his procuring sooner than he expects a subject which may show to advantage some parts at which he may not have arrived according to the order of the Syllabus; at other times from his being disappointed of a proper subject at the time he expected one.

SYLLABUS.

1. An introduction to the study of anatomy, being a general view of the composition of an animal body.
2. On the fluids in general, but particularly of the blood, with observations on some of its diseases.
3. On the solids in general, with a particular account of those fibres that are the least organic of the cellular substance; with some chirurgical remarks.
4. On the other species of solids, particularly of the ligamentous fibres, the tendinous, the muscular, and the nervous.
5. On the structure and office of the vascular system, and particularly of the arteries.
6. Of the veins in general, with remarks on diseases of the vascular system.
7. On the structure and office of the lymphatic system.
8. On the lymph contained in the lymphatic system, and on the fluid that lubricates the different cavities of the body; with remarks on inflammation and suppuration.
9. On the different glands, and the manner in which they perform their secretions.
10. On the structure of the bones, periosteum, and marrow.
11. A particular account of the appendages of bones, as the cartilages, ligaments, and synovial glands, and the sacculi minori.
12. On the classes of bones, and of the manner in which bones are formed.

2.—*On the Osteology.*

13. An introduction to the study of osteology; a particular account of the spine; remarks on some of its diseases.
14. A particular account of the composition of the pelvis, with remarks on different labours.
15. On the composition of the thorax, with some remarks.
16. On the bones of the upper extremity, with a particular account of the clavicle, scapula, os humeri, radius, and ulna.
17. On the bones of the hand and of the os femoris.
18. Of the bones of the leg and foot, with remarks.
19. On the skull in general, and the best method of examining its composition, with a particular account of its external conformation.
20. On the internal conformation of the skull, with remarks on the sutures and on the composition of its substance.
21. The structure of the face shown in a variety of sections, with remarks on the sinuses.

22. On the separated bones of the skull, particularly the os frontis, ossa parietalia, os occipitis, and ossa temporum and os ethmoides.
23. On the os sphenoides and the bones of the face, with remarks on the hydrocephalus.
24. On the structure of the teeth, with remarks on some of their diseases.
25. On the diseases of bones.

§ 3.—*On the Myology, and on the Male Organs of Generation.*

26. Of the structures of muscles and tendons, and the method of studying myology.
27. Of the muscles of the abdomen.
28. On the male organs of generation, and particularly of the testes, with remarks on hernias.
29. Of the structure of the penis and its muscles, and of the introduction of catheters.
30. On the scrotum, with a side view of the contents of the pelvis.
31. Of the structure of the bladder, vesiculæ seminales, prostate gland, and urethra, with remarks on some of their diseases.
32. On the diaphragm and the muscles of the face.

(32 to 75 missing.)

75. The structure of the organs of taste and that of smell, the lips, salivary glands, tonsils, and a back view of the fauces, with the thyroid gland.
76. On the organ of the voice and of the external ear, meatus, membrana tympani, and contents of the tympanum.
77. On the structure of the labyrinth; the theory of hearing, with remarks on some of the diseases of the ear.
78. On the different methods of injecting and making anatomical preparations.

TO CORRESPONDENTS.

SPECIAL NOTICE.—*Many readers of the* ASCLEPIAD *having requested permission to have, for framing or for the album, extra copies of the autotype portraits published with the biographies, the Author has pleasure in stating that every one who sends him intimation, to* 25, *Manchester Square, of having complied with the terms of publication as a subscriber, will receive at the close of this year separate copies of all autotypes published in the volume for* 1891.

In order to prevent the frequent and troublesome mistake of addressing communications to the "Editor," it is necessary to repeat that everything published in the ASCLEPIAD *is exclusively by the Author whose name is on the title-page.*

Books, Reports, and Pamphlets intended for notice in the ASCLEPIAD *should be addressed to the Author at his residence,* 25, *Manchester Square, London, W.*

Names of Subscribers and other Matters of Business should be forwarded to Messrs. Longmans, Paternoster Row, London, E.C.

Advertisements, and all Communications respecting them, should be forwarded to Mr. A. P. Watt, 2, *Paternoster Square, London, E.C.*

The Author will be greatly obliged if senders of Newspapers and Magazines would kindly mark the passages to which they wish to direct his attention.

The "Appendix of Acknowledgments" will appear in next number.

August 15th, 1891.

ON PEROXIDE OF HYDROGEN: A PHYSICAL-MEDICAL RESEARCH.

CONTINUED FROM A RESEARCH COMMENCED IN THE YEAR 1856.

PART IV.—PHARMACOLOGICAL AND PRACTICAL.

IN this fourth and concluding part on Peroxide of Hydrogen I have to treat on the pharmacological and practical details of my subject, so as to enable the practitioner of medicine to see at a glance the nature of this medicinal substance and the manner in which it is best to prescribe it in disease. For a few pages the reader will be invited to dwell on matters of theory and practice relating to the manufacture of the fluid itself. Afterwards he will be presented with a series of formulæ, the outcome of long experience, for the proper administration of it in the various details of practical medical work.

The Production of the Peroxide.

The theory originally advanced by Thenard has been ably condensed by Gmelin in the following passage:—"When peroxide of potassium, sodium, barium, strontian, or calcium, is digested in any hydrated acid, which forms a soluble salt with the salefiable base resulting from the

decomposition of the peroxide, the excess of oxygen does not escape as gas, but passes over to a portion of the water, and *converts it into peroxide of hydrogen.*"

By what line of experiment Thenard arrived at this curious chemical fact there is, I believe, no record; it must have been the result of long experiment, the details of which have not survived, although they indicate an advanced chemical knowledge. By common agreement the peroxide of barium, baryta, seems to have been the substance selected as best for the process of manufacture of the hydrogen peroxide, and it continues to hold its own. The baryta must be freed from all foreign matter, especially manganese, must not give out heat on being made into a paste with water, and must be entirely soluble in dilute hydrochloric acid. When pure, it is a hard substance, presenting a greyish surface free of green specks. It can be kept for any length of time in a closed bottle.

The next step of the process, after Thenard, is to take a certain measure of distilled water, *twenty fluid ounces*, and add to that quantity *two fluid drachms and twenty minims* of pure hydrochloric acid. The acid must be mixed well with the water by agitation with a glass rod. Then *two drachms and ten grains* by weight of the pure peroxide of barium, reduced to a fine powder, is to be placed in a porcelain glass or enamelled basin, and treated with a little water, until it is made into a soft paste. Next, with a glass or boxwood knife,—an iron one must never be used for the purpose—the paste is to be gradually added to the acid solution until all is dissolved, but if there remain a precipitate a little more acid must be dropped in to take it up. The first step of the process is now completed; the peroxide of barium has been transformed into the soluble chloride of barium, and,

at the same time, the water has been charged with *one volume* of hydrogen peroxide.

To charge further it is necessary to remove the soluble barium chloride, which is diffused through the water. This is done by adding to the solution, very gradually, *one fluid drachm and a half* of dilute sulphuric acid, made by mixing *one* part of the acid with *three* of water. On this the baryta is precipitated in the form of insoluble sulphate, and there is left above a solution of *one volume* of peroxide of hydrogen, with the hydrochloric acid free and ready again for action. The solution must be poured off, the precipitate filtered through Swedish filtering paper, and treated with barium peroxide as before, and so the process is to be repeated until the charge of ten volumes of oxygen is obtained, or until the water is charged with oxygen to the amount required. If over ten volumes be wanted, the stronger charge can be got by gentle evaporation of the water, and by this plan as many as *thirty volumes* of oxygen can be obtained; but here for practical purposes we must stop. Thenard, by evaporation and exhaustion with the air-pump and sulphuric acid, obtained absolute peroxide, which charged the bell-jar in a state of vapour, and yielded 475 volumes of oxygen. I spent much time, expense, and trouble, in attempting to do the same, with the object of ascertaining if the peroxide of hydrogen in the gaseous form would support life as oxygen does, but I never succeeded.

If it be desired now to remove the hydrochloric acid from the solution, so as to have at hand the solution of peroxide of hydrogen pure and simple, sulphate of silver is added to the solution gradually, upon which chloride of silver is precipitated, and the solution containing sulphuric acid is left. This solution is decanted, filtered, and treated with baryta until all the sulphuric

acid is fixed and thrown down as sulphate, and the solution is neutral to test paper. The solution once more decanted and filtered is the pure solution of the strength of volume wished for.

It is right to explain the manner in which the peroxide may be obtained in the pure state, but for pharmacological purposes it is rarely necessary to remove the hydrochloric acid. It is an advantage to allow the acid to remain, because its presence renders the oxygen more stable in the solution. The small amount of acid present is not injurious, and when it is necessary to have a neutral or alkaline solution, the gradual addition of a little pure ammonia, caustic soda, or potassa up to neutrality, is sufficient.

Simpler methods of making the peroxide have been employed. Pelouze suggested its manufacture by adding peroxide of barium to a solution of hydrated hydrofluoric acid. Fluoride of barium is thrown down, and solution of peroxide of hydrogen is formed. This has been called the "direct method," and I believe that for some commercial purposes it is rather largely employed.

Physical Properties of the Peroxide.

The properties of the peroxide of hydrogen as it exists in solution are peculiar. Some of them have been touched on incidentally in previous papers, but it will not be out of place to notice them here. The oxygen imparts no colour to the solution, but gives a distinct odour resembling that from a weak solution of chlorine; and when a little of the solution is placed on the hands this odour is most marked. To the taste the solution, even in weak solutions, is caustic and styptic, and very strong solutions of it placed on the tongue cause pain. In the strongest solution I ever succeeded in producing a few minims

of the solution placed on my lower lip turned the mucous membrane white, as if it had been frozen, and the return to the natural colour was attended by a rather sharp tingling sensation.

In its highest concentration the peroxide of hydrogen has a specific gravity of 1·452, and at 30° Fahr. it does not freeze. It mixes with water in all proportions, and may be kept in solution at all ordinary temperatures. It is best, however, to store it in a cool place, and it is essential to see that the bottle containing it is at all times firmly closed. If the stopper or cork be left out, particles of dust may get into the solution and set the oxygen free. On the whole it is best to close the bottle with a good-fitting cork, for although in time the cork bleaches and softens from contact with the solution, and even from the vapour of the solution, it retains the oxygen as securely as a glass stopper, and is easier to remove.

The action of light on the solution is negative; but heat causes the oxygen to be evolved. When the solution is heated to boiling point the extra measure of oxygen does not go off until the boiling point is reached; then the oxygen begins to evolve with the vapour of the water, and continues to go off until it is all gone, even though the water be made suddenly cool. But although heat of itself does not liberate the oxygen until boiling point is reached, it aids greatly the oxidation of various oxidisable bodies in contact with the solution, a point of considerable importance in the application of the solution for therapeutical purposes.

The electrical current passed through a solution of the peroxide of hydrogen causes decomposition in the usual way, the oxygen and the hydrogen appearing at their respective poles; but the oxygen is in larger proportion than if water simply were undergoing decomposition.

The grand peculiarity of peroxide solution is the readiness with which, under some circumstances, oxygen is evolved. This peculiarity has led me to doubt whether the solution be a peroxide of hydrogen at all and whether we ought not to consider it as oxygen combined with water as a radical, or with the oxygen of the water. To my mind it is ozonic oxygen in combination with water. It gives all the reactions of ozone while it is in combination, said reaction extending to water in finest division as spray. But when it is passing over with water vapour at a high temperature it loses its magnetic or ozonic qualities, and when it is liberated from the solution by other means and is caught in a bell-jar for examination it also loses these qualities and acts like common oxygen. This might be expected from ozone combined with water.

It would be imagined that with oxygen in excess in a watery solution all oxidisable substances placed in the solution would be immediately and rapidly oxidised. Of some substances this is true. If to the solution sulphurous acid be added, that acid is more quickly oxidised into sulphuric acid; if to the solution arsenious acid be added, that acid is converted into arsenic.

There is nothing remarkable in the changes here named. But very remarkable is the behaviour of some other chemical substances. If to the solution peroxide of manganese be added, the oxygen is liberated with the briskest action; if black oxide of platinum be added, the effect is still more striking; and the same is evidenced with the oxides of gold, silver, and mercury.

Other bodies, themselves rich in oxygen, give up their oxygen on coming into contact with the peroxide solution. This is specially notable in the case of the permanganates. Add to a solution of peroxide of hydrogen a solution of permanganate of potassa, and the action is almost ex-

plosive. The red colour of the permanganate disappears, and oxygen derived from both solutions can be collected in large quantity. I have availed myself of a knowledge of this fact in utilising the peroxide for medical purposes.

A number of curious facts came out of my earliest experiments, indicating that the evolution of oxygen by the presence of manganese and substances similar in action could be more or less checked by the interposition of other chemical substances. Ammonia, I found, had this effect, hydrocyanic acid, alcohol, and a large number of the alkaloids capable of entering into solution with water, as morphia, aconitina, and strychnine. Some volatile bodies—ammonia, for example—have this power while they are in solution, but when volatilised the effect ceases.

Internal Administration of the Peroxide.

After I had succeeded in making the first quantity of the peroxide of hydrogen on Thenard's method, and had obtained a purely neutral solution which yielded ten volumes of oxygen gas, I proceeded to experiment with it physiologically and therapeutically. In commencing the therapeutical part, it was the first duty to determine the dose and the most agreeable means of prescribing the substance medicinally. I began by making a few experiments on myself. I put one fluid drachm of a *ten-volume* solution into one ounce of ordinary water, and slowly swallowed the dose. The taste was peculiar, seeming to me metallic in character, but afterwards it suggested itself to me that it was rather like chlorine water, and on tasting a solution of chlorine this view was confirmed. I also found that when I drew ozonised oxygen freely into the mouth, with a little water in the mouth, a similar taste was produced. The taste was styptic and metallic; it affected the

posterior palate and fauces rather than the tongue, and the epiglottis was slightly irritated. The effects soon passed off.

The general effect of the fluid drachm dose not being very significant, I took on the next day *two* fluid drachms in *two* fluid ounces of water. The local effects were intensified considerably, and lasted for a longer time. A day later I took *three* fluid drachms in *three* fluid ounces of water, and a day later still *four* fluid drachms in *four* fluid ounces of water, with an additional ten ounces of water soon afterwards, the local effects being disagreeable and the stomach becoming oppressed, as from distension. This condition lasted for a few minutes, and was succeeded by a slight exhilaration, with some quickening of the pulse and an increased flow of saliva, accompanied by a peculiar taste like that which occurs under potassium iodide. These results led me to propose a ten-volume solution—since generally accepted—for medical purposes, and *one* to *four* fluid drachms, with water in the proportion of an ounce of water to a drachm of the solution, for the ordinary dose.

On the whole I have held to the ten-volume solution as the most practical. It is more stable than the solution of greater volume; it fits in well in prescribing for internal use, and for solutions for external use—gargles, lotions, and injections—it meets, generally, every purpose. Moreover, it gives a uniform or standard solution, which is a great advantage; and I recommend members of the profession, when they order the peroxide, to mark this strength by adding the words "ten volumes" to the prescription.

One fluid drachm to four of the ten-volume solution is the dose for adolescents and adults. For children under fifteen the dose runs from thirty minims to two drachms, diluted with an agreeable quantity of water.

The solution can be combined with simple syrup, with syrup of Tolu, and with some emollient drinks, notably with barley water, a menstruum in common use with me for this purpose. But the most convenient sweetening menstruum is glycerine in combination with water. One drachm of glycerine to four drachms of the solution and the proper amount of water is the proportion I most commonly prescribe; but in phthisical cases I often increase the dose of glycerine up to two and three drachms.

The solution must always be kept slightly acidified with hydrochloric, sulphuric, or phosphoric acid; but when prescribed in this form the presence of the acid must always be remembered. Sometimes the acid may be retained, with advantage; sometimes it may, with advantage, be neutralised by the addition of solution of soda, potassa, or ammonia; but in such neutralisation the carbonates of these bases must not be used, since the escape of carbonic acid causes liberation of the oxygen.

Some Useful Formulæ for Internal Administration.

I will now add a few useful formulæ for the service of those who are not familiar with the solution in the prescription. The dose ordered is the full medium for adults:—

Astringent and Oxygen Mixture.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Acidi Sulphurici Diluti, ʒjs.; Glycerini Puri, ℥js.; Aquæ Destillatæ ad ℥vj. Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

This forms an excellent mixture in cases where the peroxide is combined with an astringent, as in cases of colliquative sweating in Phthisis pulmonalis. Each dose of mixture should be diluted with six or eight ounces of water or barley water.

Mixture of Quinine and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Quinæ Disulphatis, gra. vj.; Acidi Hydrochlorici Diluti, ♏ xx.; Glycerini Puri, ℥js.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

A good mixture in cases where it is desired to combine quinine with oxygen, as in influenza and in febrile remittents. The quinine can be increased up to five grains per dose if necessary, and dilute sulphuric acid can take the place of the hydrochloric if the prescriber wishes. Each dose should be diluted in six ounces of water, cold or iced.

Mixture of Tannin and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Tannin, ʒjs.; Glycerini Puri, ʒvj.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

A very useful prescription in hemoptysis and in other instances in which there is escape of blood. If required, diluted sulphuric acid can be added in proper proportions. Each dose should be diluted with water, cold or iced, before administration, and the mixture should be kept in a cold place, since it has a tendency to undergo decomposition.

Mixture of Morphine and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Liquor. Morphiæ Hydrochlor., ʒj.; Syrupi Tolutani, ʒvj.; Glycerini Puri, ℥ijs.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

The morphine and oxygen mixture is of much value in cases of severe cough with expectoration, and with sleeplessness in consequence of cough, as in chronic and asthenic bronchitis, and in phthisis. The dose of morphia can be increased to any required amount, and mineral acid can be added if an astringent be desired. The adminis-

tration should be by dilution in cold or iced water, as before.

Mixture of Codeine and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ʒijs.; Syrupi Codeinæ, ʒij.; Sp. Vini Rect., ʒvj.; Glycerini Puri, ʒvj.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

This form, for diabetes, has been already reported upon (Asclepiad, No. XXX.); but glycerine is here introduced as a good addition. The amount of syrup of codeine can be increased if necessary.

Mixture of Phosphate of Iron and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ʒijs.; Acidi Phosphorici Diluti, ʒj.; Syrupi Ferri Superphosphatis, ʒvj.; Glycerini Puri, ℥j.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

The best combination I know of for asthenic cases in which iron, phosphates, and oxygen are indicated. I have prescribed the mixture during the early stages of Phthisis pulmonalis with special advantage. When the circulation is rapid and feeble, five to ten minims of tincture of digitalis may be added to each dose. The mixture is serviceable also in the anæmia of strumous subjects.

Mixture of Iron, Quinine, and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ʒijs.; Acidi Phosphorici Diluti, ʒj.; Quinæ Disulph., gra. vj.; Syrupi Ferri Superphosphatis, ʒvj.; Glycerini Puri, ℥j.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

The above form is serviceable in cases where quinine and iron with oxygen are likely to be suitable. If a sedative be called for at the same time, a proper dose of solution of hydrochlorate of morphia—five to ten minims—is admissible and compatible.

Mixture of Quinine, Iron, Strychnine, and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Acidi Phosphorici Diluti, ʒj.; Syrupi Ferri Superphosphatis, ʒvj.; Quinæ Disulphatis, gra. vj.; Sp. Vini Rect., ʒvj.; Liq. Strychniæ, ♏ xxiv.; Glycerini Puri, ℥j.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

This mixture is of service in affections where Easton's syrup is useful, and where the addition of oxygen promises well. In anæmic conditions the addition of the oxygen is of considerable benefit. I originally prescribed the peroxide with Easton's syrup direct, but the combination was not satisfactory; hence the new form here prescribed. The action of the strychnine is somewhat increased in this mixture, and requires, therefore, to be carefully watched. The dose may be taken twice daily in six or eight ounces of water.

Mixture of Alcohol and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Spiritus Vini Rectificati, ℥iij.; Glycerini Puri, ʒvj.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

The peroxide solution mixes readily with alcohol, and the above combination supplies a mixture of great value in cases where an antispasmodic is required in combination with oxygen, as in asthma and in some stages of collapse, where the breathing is oppressed. This mixture, moreover, may be accepted as a spirituous solution of peroxide, and may become the menstruum for other substances that are soluble in alcohol and do not readily undergo decomposition in the presence of oxygen. The mixture must be administered in water in the same proportions as already directed, and the water may be warm, cool, or cold (iced), according to the judgment of the practitioner.

This mixture may also be given with barley water or

thin jelly, or with coffee. I have seen benefit of marked kind from it in asthma, angina pectoris, colic, and cramp of the lower extremities.

Mixture of Alcohol and Amyl Nitrite with Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ʒijs.; Spiritus Vini Rectificat., ʒiij.; Glycerini Puri, ʒvj.; Amyl Nitritis Puræ, ♏ xx.; Aquæ Destillatæ ad ʒvj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

The properties of this mixture are similar to those of the last, but the presence of the *amyl nitrite* renders it more decidedly antispasmodic in its action. The dose may be administered in six or eight ounces of water; but the water should always be cold, and the dose should be sipped slowly, never swallowed with a gulp, according to the common practice of swallowing medicinal substances.

N.B.—It is advisable to prescribe this mixture in limited quantities, say of six doses at a time, and not more, as the nitrite is disposed to undergo decomposition if it be retained long in the presence of the oxygen.

The combinations of alcohol with oxygen solution go well with ether, and produce a good antispasmodic compound, but I usually prefer, in prescribing the solution with ether, to use *ozonic ether*.

Ozonic ether, to which I have more than once alluded, is the peroxide in ether. I lighted upon it very early in my researches on shaking up a strong peroxide solution with anhydrous ether. It is made by admixing a thirty-volume solution of the peroxide with anhydrous ether, adding at the same time 5 per cent. of alcohol. It is a fairly stable compound. It can be administered in water in doses of half a drachm to two drachms, and can also be administered locally in the form of spray.

Ozonic Ether Mixture.

℞ Ætheris Ozonici; Spiritus Vini Rectificati; Glycerini Puri, āā ʒvj.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

In spasmodic breathing, as in pertussis and spasmodic asthma, this mixture is of much value. It is compatible to add to the mixture solution of ammonia (*fortior*) in two to five-minim doses if necessary. In the case of children suffering from pertussis, I usually dispense with the alcohol and prescribe as follows:—

Simple Mixture of Ozonic Ether.

℞ Ætheris Ozonici, ʒiij.; Syrupi Tolutani, ʒvj.; Glycerini Puri, ʒiv.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

There is, I believe, no remedy more effective in pertussis than this mixture. It should be administered in ice-cold water.

Mixture of Alcohol, Creosote, and Oxygen.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ℥ijs.; Spiritus Vini Rectificati, ℥iij.; Ol. Creosoti, ♏ vj.; Glycerini Puri, ʒvj.; Aquæ Destillatæ ad ℥vj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

This mixture is antiseptic, deodorant, and antispasmodic. I have prescribed it with best results in cases of irritable stomach, attended with sour eructations and vomiting of fermenting matter. In one instance of this kind in which malignant disease of the stomach had been diagnosed, recovery took place in fifteen days, as if some process of fermentation of a putrefactive kind had been stopped. The medicine was administered in iced water three times a day, with intervening light meals of peptonised milk, minced fowl, also peptonised, and distilled ice-cold water, bread and other amylaceous substances being forbidden.

PEROXIDE MIXTURE FOR SALIVATION.

℞ Solut. Hydrogenii Peroxydi (10 vols.), ʒiijs.; Liq. Potassæ, ♏ xxx.; Glycerini Puri, ʒvj.; Aquæ Destillatæ ad ʒvj.; Misce. Fiat Mistura. Dosis, pars sexta, modo præscripto.

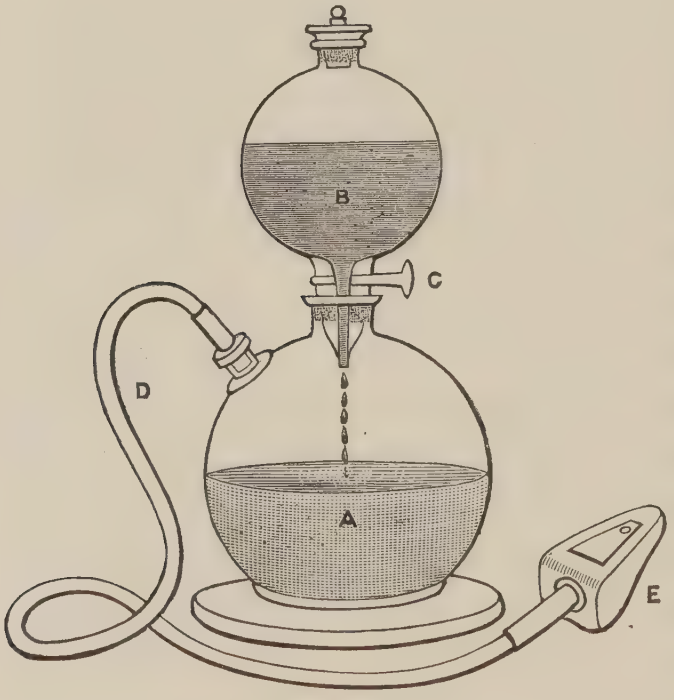
The mixture can be administered freely diluted with water three times a day, and the quantity of the peroxide can be increased, if necessary, until the effect is produced. The patient becomes conscious of the salivation by the circumstance of detecting, as under iodine or mercury, a metallic taste, and of retaining the taste for a considerable period after each dose. He may also experience a sense of restriction in the pharynx, with oversecretion there. The increase of saliva is not so profuse as in salivation from mercury, and the gums are not inflamed; but I have seen occasionally a rather profuse flow of saliva, lasting for some days. On withdrawing the medicine the local effects soon cease. It is best practice to withdraw it by degrees, so as to keep up the effect of elimination for a week or even ten days. The action is so like that of mercury, it is sometimes difficult to see the difference except in the absence of swollen gums and the more rapid subsidence of salivation on withdrawal of the medicine. The salivary glands may be rendered tender, on which the dose should be reduced.

The use of the peroxide for elimination up to salivation is best described as applicable to the class of case in which mercury or iodine would be employed. It is a substitute for these remedies, but it may be used in conjunction with them, to sustain or to quicken their influence. It does both, and renders small doses either of iodide of potassium or of Plummer's pill more effective than larger doses of them taken alone, an important advantage.

In prescribing the iodide or mercury with the peroxide, they (the iodide or mercury) should be taken at different times. I usually prescribe the Plummer's pill at night, and the peroxide solution three times during the day, when the peroxide is not effective enough of itself, and when the symptoms are very persistent.

Administration by Inhalation.

The oxygen from the peroxide can be administered by inhalation by means of the simple apparatus depicted below, which I have constructed for general use, and in working which I have taken advantage of another oxygenated body, that liberates the oxygen and at the same time yields up its own oxygen. The large glass vessel (A) is for holding the solution of the peroxide. A tube having a stop-cock (C) runs through the stopper of the vessel from a smaller glass vessel above (B) for containing the liberating solution. From the larger bottle there springs tube (D), at the free end of which is a double-valved mouthpiece (E)—shaped like the oldest and best of the chloroform inhalers—for inhalation. When the apparatus is wanted for use, a little less than half the lower vessel (A) is charged with the peroxide solution, and the upper one (B) is filled with the



displacing solution, which consists of a saturated solution of permanganate of potassa. The tap of the stopcock is then turned, the permanganate solution drops into the peroxide solution, rendered of acid reaction by a little dilute hydrochloric acid, and immediately oxygen is briskly evolved. This if necessary can be caught in a flexible balloon or bag, from which it can be inhaled in measured doses, but it is quite as well to inhale it directly, and to regulate the supply by the mode in which the permanganate solution is admitted into the lower vessel. The solutions neutralise each other, and while there is no colour in the lower solution oxygen is being evolved. When colour is shown more of the peroxide is required to be admitted.

Should the practitioner desire to combine the oxygen with ether, or with any other volatile body soluble in ether, but not in water, he can pour the ether on the top of the peroxide solution in vessel A. Then, as the oxygen escapes, the ether, or the ether with its other volatile comrade, passes over, in combination, with the oxygen. Nitrite of amyl can be inhaled in this manner with ether; and, in short, there is here a means of administering most of the active volatile medicaments by inhalation, and in a form most ready for combination with the blood as it passes in its course over the pulmonic circuit.

The oxygen in passing through the ether reassumes ozonic properties, and the continued taste of the ether informs the patient that the action is being continued. A patient can help himself to a dose at any time as may be directed.

This mode of administration of the oxygen from the peroxide and permanganate solutions is of all others the most refined and effective. It is like a new instrument of treatment in Phthisis pulmonalis, in asthma, in angina,

in asthenic bronchitis, and other allied conditions in which oxygen combined with a sedative is called for. It was from this apparatus the patient suffering from pleural abscess described in last ASCLEPIAD inhaled with such rapid benefit the *ethereal ozone*, as I would designate it.

ANÆSTHESIA FROM ETHEREAL OZONE.

By increasing the surface of ether in the apparatus depicted above and forcing it over by the oxygen liberated with permanganate, sufficient ether can be diffused with the oxygen to produce general anæsthesia by inhalation. In administering the ether in this manner, it is not necessary to remove the mouthpiece from the face of the patient; he is receiving a supply of oxygen and ether that can be regulated in amount by the supply of the permanganate solution. Ten ounces of the peroxide solution of twenty-volume strength are sufficient, with the additional oxygen from the manganese, to diffuse steadily into the mouthpiece from one to two ounces of anhydrous ether for inhalation. I am prepared to see this mode of administration of ether and of analogous anæsthetics become of general application. Such anæsthetics must be capable of floating on water, as well as insoluble. Chloroform and methylene sink to the bottom and diffuse too slowly.

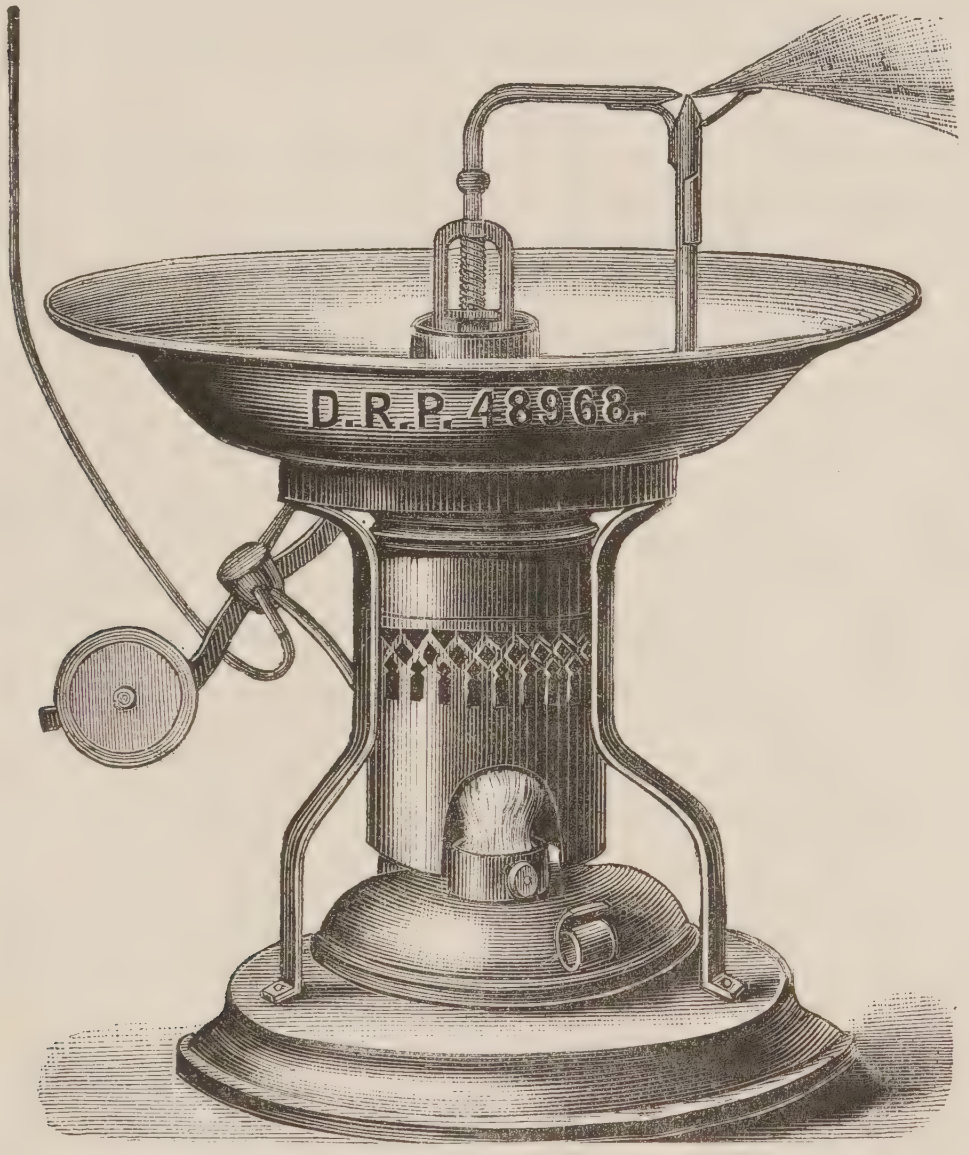
ADMINISTRATION IN FORM OF SPRAY.

Spray for the Sick Room.

Reference was made in a previous paper to ozonic or sea spray for the sick-room. It is a solution diffused as spray from a spray producer. The solution is composed as follows:—

Solution of peroxide of hydrogen (10-volume strength), 10 ounces; common salt, 2 drachms; Tincture of iodine, 1 fluid drachm. To make a solution for spray.

If a pleasant and pine-tree odour be desired, a few grains of *Kaurie gum* can be added, and the effect can easily be produced. In sick-rooms and in the wards of hospitals such sprays can be arranged to work automatically so as to deliver and diffuse the solution at regular intervals.



For this purpose Messrs. Krohne and Sesemann have constructed for me an apparatus which will distribute spray for several hours at a time.

For a styptic and deodorant spray for the throat the following form is useful :

Rectified spirit, 1 fluid ounce; Tannin, pure, 20 grains; Distilled water, 1 fluid ounce. Dissolve the tannin in the alcohol and water, and

add Solution of Peroxide of Hydrogen (10-volume strength) to make 6 fluid ounces. To make a solution for spray.

Of this solution one or two fluid drachms can be used with Siegle's steam spray inhaler, a mode especially applicable in case of throat affection. Another very good spray solution is made by substituting *boracic acid for tannin*, or if a styptic be required, adding the tannin as well.

When the peroxide spray is brought into use it is good practice, in some cases, to make the spray the means of administration, in small doses, of medicinal agents which may act internally. Some mercurial preparations may be administered in this manner, and also soluble preparations of opium.

It may be added that peroxide solution goes well with alcoholic solutions containing many agents, soluble in alcohol, and afterwards miscible with water. In combination with it, in this manner, most of the essential oils and various ethers can be administered.

Application in the Form of Gargle, Collyrium, Lotion, or Ointment.

Gargle.—The peroxide solution can be prescribed as an antiseptic and astringent gargle. A good form for this purpose is:

Tannin, pure, 10 grains; Glycerine, pure, 1 ounce; Rectified spirit, 4 fluid drachms; Distilled water, 4 fluid ounces; Solution of peroxide of hydrogen (10 vols.), sufficient for 8 fluid ounces. To make an eight-ounce gargle, to be used in the manner directed.

A rapid mode of treating the fauces in ulceration and in the diphtheritic condition consists in sponging the affected part first with solution of permanganate—Condy's fluid will do—and following that up by another sponge charged with the solution of peroxide. The escape of

oxygen is very free, and the solution must be applied with care so as not to provoke cough.

Collyrium.—The peroxide is useful as a collyrium, especially in cases where there is purulent secretion. The following is a good combination :—

Solution of peroxide of hydrogen (10 vols.), 4 drachms; Tannin, pure, 5 grains; Rosewater to 8 fluid ounces. To make an eight-ounce collyrium, to be used in the manner directed.

It will be observed that the quantity of the peroxide solution prescribed above is very small. This is recommended because the solution is apt to give smarting and pain at first if it be applied too freely; but the quantity can be increased so soon as the effect of it is determined.

Lotion.—An evaporating lotion can be made with the peroxide in the following form :—

Rectified spirit, 4 fluid ounces; Anhydrous ether, 1 fluid ounce; Distilled water, 4 fluid ounces; Solution of peroxide of hydrogen (10 vols.) to 12 fluid ounces. To make a lotion of twelve fluid ounces, to be used as directed.

Ointment.—A simple yet useful preparation of the peroxide is as an ointment, for inunction over the surface of the skin after scarlet fever, and other affections where there is free shedding of dead cuticle. To make the ointment—

Rub up ozonic ether with spermaceti ointment, so as to form a thin paste, which may, if desired, be rendered thick by the addition of a little more spermaceti, or thin by addition of olive oil.

Conclusion.

In closing these pharmacological notes it is well to recall the important fact that the remedy we have had before us has about it the peculiarity that it is a purely natural remedy, that it is not toxic, and that, with the exception of the danger of producing by it gaseous accumulation in the

blood under some circumstances of its use—a danger which admits of being avoided—it is free of all risk. In its remedial application, too, there is no complexity; it is always oxidation. For asphyxia it is oxidation; for spasm it is oxidation; for breaking up pus and exudations it is oxidation; for destroying infectious and fœtid organic secretions or excretions it is oxidation; for producing eliminative action it is oxidation.

At the close of the paper on peroxide of hydrogen read before the Medical Society of London in 1860, I said:—

"Sir, in placing this memoir on the annals of the Society I guard myself, once and for all, from any exaggerated suggestions as to the value of this new medicinal agent. The subject is so novel, even to me after twelve months' learning and knowledge of it, I have feared to use a sentence that has not been measured and recast over and over again. I am not offering this medicine as an elixir vitæ. I do not pretend to know all its properties. I do not bind myself, irrevocably, to that which, on the present occasion, has been offered; on the contrary, I remain open to all new observation and knowledge for correction and guidance. I believe we have here within our grasp a powerful medicine, the full value of which it may take years to develop, and the useful application of which may be developed in directions different from those now advanced. My purpose will be served if I but call forth investigation and elicit fact; let the course of the events bend as they may."

A generation has passed since these words were spoken. I repeat them with more knowledge, more experience, and more confidence, but still expressing the same earnest hopes, the same candour, and the same respectful confidence in the voice of the future.

THE PHYSIOLOGY OF TEARS.

NO subject has yielded more poetry than that of tears. Adjectives the most various have been used to express the qualities of tears. They have been called "hot," "cold," "languid," "briny," "gushing," "round," "glittering," "dull," "silent," "wearied," "wanton," and I know not what else. They are waves of emotion, and as a general expression they are said to spring always from the heart, an expression singularly truthful, for no one ever wept from the head; that is to say, no one ever reasoned himself or herself into tears except through an appeal back to an emotion. There are very few persons who do not under some emotions shed tears, and it is probably quite true that they who can always restrain them are, according to the common opinion, of a hard and unimpressionable nature. The statement often made that insane persons do not shed tears is not all true, but there is some truth in it. I have seen the insane weeping, but I must admit that on visiting the wards of great asylums there is a remarkable absence of weeping in comparison with the noise, irritation, and wandering of intellect that is forced on the attention. Also, I have known a sane person who became insane, owing to a great calamity of grief, overwhelmed with weeping while the sanity remained, but perfectly and persistently tearless when the

insanity was manifested, a result that may appear natural when the physiology of weeping is properly understood.

The poets have dealt with what may be considered, from their point of view, the chemistry of tears. They have written about "salt tears," "crystal tears," "poisonous tears," "honeyed tears," and the like. They are to some extent correct in their poetic guesses. Tears are saline, for they carry a trace of the chloride of sodium or common salt which exists in the blood; they may be poisonous, for in persons under the influence of a soluble poison, like a salt of antimony, they may convey an infinitesimal part of such poisonous material; they may be honeyed, for in diabetes the tears do sometimes become saccharine like the other fluid secretions. But this sort of thing is exceptional. In a general way tears are made up of pure water holding a trace of saline matter, and nothing more; the glands which secrete them are the purest wells of pure water in the body.

Tears are the result of a nervous storm in the central nervous system, under which there is such a change in the vascular terminals of the tear-secreting glands that excretion of water from the glands is profuse. Some excretion is always in process in order that the surface of the eye may be laved and cleared of foreign matters which may come in contact with it; but the controlling centre is at a distance. As the muscular power that extends or flexes a finger is at a distance from the part moved, so the excitement to tears is from an irritation in a distant nervous centre, and is removed when the nervous centre is either soothed or exhausted. The persons who weep say that tears afford relief. Nothing is more perfectly true, nothing more clear when the facts are understood. The relief comes, not from the mere escape of tears, which is only a symptom, but from

the cessation of the storm in the nervous chain. If the storm be calmed by soothing measures, as when we soothe a child that is weeping from fear, annoyance, or injury, we quiet the nervous centres, upon which the effect ceases. In children the soothing method succeeds, and sometimes it succeeds in adults, although in adults the cessation of tears is more commonly due to actual exhaustion following a period of nervous activity. In grief, the afflicted weep until they can weep no more; then they become calm, or, like children, cry themselves to sleep. Thus tears indicate relief, and that the nervous system has fallen into the repose of weariness. Persons subjected to many and repeated griefs shed in time fewer tears, and the aged compared with the younger are almost tearless. The poor insane patient who ceases to weep becomes griefless; under the continued excitement the grief centre fails or dies. If this were not the case tears would flow in such a person so long as life lasted. Tears have their value in the life of mankind; they are of value not as tears, although their actual flow gives relief, but as signs that the grief centres are being relieved of their sensibility, and that the nervous organisation is being fitted to bear up against sorrow.

I crossed the Thames in a boat at Putney with a man eighty-four years of age. He told me, "It is sixty years since I last made this passage in this same place, and then it was to fetch the famous Dr. Hooper" (author of Hooper's *Physician's Vade Mecum*) "to see my child Tom lying at death's door with scarlet fever. I was so heart-broken, and cried so terribly, that the men in the ferry-boat, which then plied here, tried to console me. Tom recovered and lived until last year, when he went before me. If he had died of the scarlet fever when he was young, I veritably believe I should have died

too of tears and grief, and yet when he died fifty-nine years later, during all which period he and I had been affectionately attached to each other, I could not shed a tear, nor could I again feel the poignancy of that early grief. I accuse myself of being without feeling, and yet I cannot help it. Can you doctors explain the reason?" We explain it as above, and we think the explanation as merciful in fact as it is clear in theory.

Respecting the nervous excitation which calls forth tears, I have noticed how little it is due to physical pain. It is called forth by fear, by anxiety, by affection, by grief, but not even by pain extending to agony. In the days preceding the use of anæsthetics I have seen patients who were undergoing surgical operations faint; I have heard them cry out and scream until they made the bystanders sick and pale, but they rarely, if ever, shed tears. The parturient woman in the acme of her "great pain and peril" may suffer the extremest physical agony, under which her cries are piercing, but she rarely sheds tears. Indeed, I never recollect seeing the most nervous of her class, under such circumstances, shedding a tear. Strangely, however, during the sleep induced by an anæsthetic like chloroform or ether, I have seen profuse tears, not from suffering, but from some emotional dream induced by the narcotic.

A very slight emotional disturbance will induce the nervous irritation leading to tears in susceptible subjects; and this although the catastrophe has nothing to do, intrinsically, with the person affected. Hence the commotion of tears conjured up in a play. Hamlet, it will be remembered, seizes aptly this point when the player weeps. "What's Hecuba to him, or he to Hecuba?" Of course nothing, yet the player weeps, and maybe the audience weeps with the player. By art another

remembrance may be used to call forth tears on the proper occasion. A well-known player was asked how he managed to weep when he willed. He replied, "I call up the remembrance of my dear father, who is dead." On the other hand, anything that produces diversion of mind, when the disturbance is not severe, may keep back the outbreak. John Hunter tells us that once when he went to the play to see Mrs. Siddons perform, in a moving exposition of her great powers, he could not join the rest of the house in their tears "because he had forgotten his pocket-handkerchief;" and a friend of my own, an emotional man, told me that at a funeral, where he expected to be overwhelmed with tears, he was completely checked by an absurd reading which the parish clerk gave to a sentence of the service. In these facts there is nothing incompatible, because the more intense the nervous vibrations, the more easy is the diversion of impulse from one centre to another.

As a rule the escape, and free escape, of tears relieves the heart and saves the body from the shock of grief. Tears are the natural outlets of emotional tension. But there are exceptions to this rule, and I have more than once seen uncontrollable weeping followed by serious systemic disturbance, affecting principally the heart and circulation. I have known intermittency of the heart induced in this way and assume the most serious character.

Change of scene, mental diversion, and outdoor life are the best remedies for the tearful, but an opiate judiciously prescribed is often the sovereign remedy. Other narcotics are injurious. Alcohol, so often resorted to, is fearfully injurious. It disturbs and unbalances the nervous system, keeps up a maudlin and pitiful sentimentality, and sustains the evil. Alcohol is the mother of sorrow. There are other narcotics which are similar in effect, notably chloral;

but an opiate given at night-time, under necessity, not only soothes, but controls, and when prescribed so that the use of it shall not pass into habit, is a divine remedy.

It is one of the most curious of natural phenomena that mere imagination can excite the nervous system into the production of tears, without any external aid or reflex. The readers of a pathetic chapter of a fiction may weep over the pages without wonder, because the cause of tears has been raised before the mind; but is it not strange that the writer who made the fiction may be enforced to weep over it, and cannot, in fact, expect any one else to weep over it unless he is himself affected by the picture he has drawn? Such facts place mind or the immaterial essence of humanity over and above, and over and preceding, our material nature and all its functions. They lead us to the suspicion that we might exist, think, and feel, without this mortal coil at all; or, as if, at best, the mortal coil were nothing more than an instrument, doing the bidding of the *ego*, clumsily or refinedly, according to its form, and no more; as if there might be material forms of finer build; angelic forms, as the old books have it, amongst whom tears are unknown.

I have often wondered whether the lower animals shed tears as men and women and children do! It is in the order of physiology to suppose that every animal that secretes a fluid for the laving of the forepart of the eyeball might, under emotion, throw out an excess of secretion, and shed tears; and I have often heard sympathetic people and children say of animals they were fond of, that such animals wept on the loss of those to whom they were attached, on deprivation of some pleasure or on receipt of punishment. This statement is made most frequently in regard to dogs, but I could never, in my own observation, arrive at evidence sufficient to confirm it.

Animals cry, men weep, would be my interpretation of the argument, and well it is so, for if animals wept at the harsh treatment they are subjected to by nature, as well as man, there would soon be none to weep. That some of them have emotions and moments of sorrow cannot be doubted; but they have no continued memory of the emotion, no recurring pictures that recall it. They forget even their offspring after the briefest periods, and they do not weep from physical pain. The picture of the tear in the eye of the hunted deer is pathetic, and by the reflex may make some weep. But the tear is the sweat of fatigue, not the sign of emotion.

As tears are secreted by glands which lie between their nervous centre and the mucous surface of the eyeball; as they have two functions or duties, one the function of relieving nervous tension, the other of laving the eyeball, so these functions may be called into overaction by internal nervous impulses or vibrations, and by external excitations or what are sometimes called reflex actions. The first is seen in the act of weeping under emotion, the vibration starting in the nervous centres, and extending to the gland from behind, urging it to action; the second is seen in the act of shedding tears from direct irritation of the mucous surface of the eyeball, as when an irritating substance "gets into the eye," and the vibration extends from the mucous surface back to the gland, exciting it to action and causing emotionless tears. In these ways tears afford a good illustration of the mode in which the nervous fibres are capable of conveying to a secreting organ exciting impulses from both sides of a gland lying in their course, and having in connection with it afferent and efferent communications. In both cases the exciting impulse is a vibration; and as when the impulse sets forth from a mere external irritation, as from a particle of dust on the

conjunctiva, the effect is in the truest sense mechanical, so, probably, in the case of the emotional irritation, which calls forth tears, the process is as purely mechanical.

In the human animal tears are most easily wrought where the sympathetic nervous system is most developed and most impressionable, and when the three great emotions, fear, grief and joy, are most active. Hence women generally are more given to tears than men, and under the peculiar state called hysteria, in which the nervous system is at highest tension, are often seen moved to tears by the three emotions, in turn, during one paroxysm.

It has been a matter for discussion why tears are always being secreted in health for the lubrication and laving of the eyeball. That this constant process is active is shown by the copious loss of tears over the cheek whenever the lachrymal duct, by which the secretion is naturally borne away from the conjunctival surface to the nasal cavity, and so to the throat, is closed up. But what keeps up the secretion? I suspect the small trace of saline substance that is present in the tears is the cause. The salt keeps up just a sufficient irritation to give an impulse back to the gland, a reflex, sufficient to sustain the necessary function. During sleep, when the eyes are closed, and there is little evaporation from the conjunctival surface, this irritation will be subdued, and tears will cease like other functions. So sleep cures tears, as it "knits up the ravelled sleeve of care."

OPUSCULA PRACTICA.

"There are mites in science as well as in charity."
BENJAMIN RUSH.

CRANIAL AUSCULTATION IN CASES OF VERTIGO AND NOISES IN THE HEAD.

FOR some years past it has been a practice with me to carry out what may be called cranial auscultation in cases of vertigo with or without singing noises in the ear or in the cranium. I was led to the practice by a case in which a cardiac murmur was distinctly traceable, by the stethoscope, along the carotids up to the temporal bone, the patient himself being conscious of the murmur at all times, but particularly when his head rested against a solid substance, or even a semi-solid substance like a pillow. Afterwards another patient visited me complaining of nothing more than noise in the head, so loud, he said, he wondered other people near to him did not hear it. I applied the stethoscope to the cranium on the side where he said the noise was most distinctly heard, and there I could hear it as clearly as he himself, a loud and painfully whizzing sound in almost continuous note. To him the sound was continuous, owing probably to the fact that it was, so to say, closer to him than to me; to my ear it was not quite continuous, but broken by the stroke of the

heart. In this instance, however, there was no murmur along the blood-vessels either arterial or venous and no cardiac murmur whatever. The patient came to me from my late friend Dr. Thomas Gray, of Essex Road, and after being under our treatment for a few weeks he went into one of the hospitals, where we lost sight of him, and I am unable to report how his symptoms terminated. In this instance there was no vertigo, but the persistency of the noise made life miserable, rendered sleep difficult, and led to dreams of the strangest and most distressful character.

There was under my observation for several years Mrs. D., a middle-aged lady, very stout and tall, who had to work industriously at a sedentary occupation. She was naturally of an active nature and happy disposition, and being very provident, had no cause for mental depression. Rather rapidly, soon after she had passed the grand climacteric, she became extremely anæmic, and applied to me on that account partly —for an increasing paleness alarmed her—but chiefly for a continuous and distressing noise in the head on the left side, which gave her no rest by night or by day. On auscultation there was audible from the heart a soft anæmic murmur connected with the first sound, and not traceable along the arteries. In the jugular veins there was a distinct venous murmur, greatly increased, as is usual, under the pressure of the edge of the stethoscope; and, from the cranium over the mastoid, on the left side, and extending over the squamous portion of the temporal, a murmur identical with that described by the patient was most clearly distinguishable. This murmur was influenced in a certain manner by various actions and conditions. When the patient was in the recumbent position it was reduced in quality of tone

and intensity; in the erect position of the body it was louder; after brisk walking and shortly after taking alcohol it was much intensified and almost continuous, in all of which phases it followed the state of the circulation. The continued and increasing force of this murmur drove the sufferer almost to despair; she consulted every one of whom she was told, qualified or unqualified, under the common idea that where things are so bad that nothing can be worse every chance of relief, however ridiculous, should be tried. In the end relief came of itself, and life has been rendered more tolerable; but, at times, there is still recurrence of the attack in a modified degree.

I have had quite recently under observation a distinguished American brother suffering from a continuous murmur in the left ear. In his case the murmur is never absent in waking hours, and is attended now and then with vertigo, but never with vomiting. The presence of the murmur renders the hearing confused on the left side, but there is really no deafness in the strict sense of that word, and Sir William Dalby, who has also been consulted, reports the external and middle ear healthy in this gentleman. Here, again, the murmur is distinctly audible. On physical examination I found a faint aortic murmur which could at times be followed in the course of the carotids, and which was audible on cranial auscultation over the mastoid process. In this position it was a continuous murmur whilst it lasted—*i.e.*, not pulsative; but occasionally it ceased objectively altogether, though subjectively it remained.

My case-books would supply other examples like the above, but these are sufficient for the purposes I have in view of indicating, in the first place, a very useful line of practice, in cranial auscultation; in the second place,

of correcting the rather common error of seizing too rashly as a cause for noises in the head, with or without vertigo, the idea of a labyrinthine origin, and of classifying all cases of the kind, in a general and unsatisfactory manner, under the head of "Menier's disease."

DISTINCTION BETWEEN ARTERIAL CRANIAL MURMURS AND LABYRINTHINE DISEASE, WITH NOTE ON TREATMENT.

THE patients referred to above had been pronounced sufferers from labyrinthine disease. They were really all sufferers from arterial changes. The noises they heard, and which could be objectively reached, were arterial murmurs. In those instances where there was a cardiac murmur the murmur was carried along the arteries. In the example where the intense murmur could be detected over different portions of the cranium, there was, probably, aneurism of a vessel on the left side of the circle of Willis. In the other instances the murmur was probably caused by the friction of blood through the artery in the carotid canal. By passing a small india-rubber tube through the carotid canal of a skull, and injecting a column of fluid, in pulsation, through it, I found it easy to induce a murmur under different conditions of tension and pressure, and when we remember the position of the canal, how near it is to the labyrinth, and what good conducting substance is at hand, one is rather inclined to wonder that a pulsating noise is not more frequently heard. A little roughness of the artery on its inner surface, an undue thinness of blood, an undue tension of the vessel in the restricted canal, must be causes, at times, of murmur.

NOTE ON TREATMENT.

The study of this subject has its practical meanings in the matter of treatment. I am quite sure that when the murmur or noise of which the patient complains can be objectively detected it is very bad practice to tease the ear or the throat with injections of water or air, or in any way to irritate the outer or middle ear. Many practitioners are of opinion that when there is mischief in the labyrinth local interference is best avoided, and the view seems, *a priori*, common sense itself. I have not sufficient clinical knowledge of such cases to enable me to speak on the point with any authority, but I can speak on the forms of vascular affection which come under cranial auscultation. In these instances local treatment is, as a rule, worse than useless. But there are different lines of rational treatment, according to the systemic condition, that are of eminent service: in anæmia, iron and purgatives; in specific cases, an iodide or hydrogen peroxide with iron; in all cases when the pulsation is severe, a sedative the best formula for which up to the present time is:— dilute hydrobromic acid, *fifteen* minims to *twenty*; infusion of digitalis, *half a fluid ounce*; and distilled water to make up an ounce and a half for the dose.

TRANSFUSION IN COLLAPSE.

SOME able communications by Messrs. Arbuthnot Lane and Francis Heatherley in the *Lancet* of September last recalled to mind some of my early observations and experiences on the effect of saline solutions injected

into the venous circulation in states of collapse. The phenomena of temporary resuscitation following upon such injection are amongst the most remarkable on record. In a case of Asiatic cholera treated by the late Mr. Henry Ancell, Dr. Rose Cormack, and myself, a woman of middle age sank in the collapse into complete insensibility. In this case we injected through a vein in the arm two pints of a saline solution, after Stevens' original plan, with the astounding effect that the patient became quite conscious, sat up in bed, dictated a short will, and signed it in proper form. An hour later, the purging having returned, we repeated the injection with the same phenomena of resuscitation, and at my suggestion the injecting needle was left in the vein, with the syringe at the other end of the injecting tube filled with saline solution, and placed in a hand-basin of water kept at blood heat, so that a current of injection could be persistently sustained. I had assumed it possible that the circulation might in this way be sustained with the blood in motion, but the trial showed the singular fact, that there was no rallying effect at all from the injection so long as the current was pushed gently. When, however, we forced on the solution so strongly as to put the vessels into tension, the signs of reanimation returned. They returned with decreasing activity no fewer than six times, but were always followed by the choleraic discharge and collapse, so that death closed the scene about thirteen hours after the first injection. In my experiments on resuscitation by injection of heated water into the arteries of dead lower animals, I observed the same necessity of vascular tension for producing symptoms of reanimation.

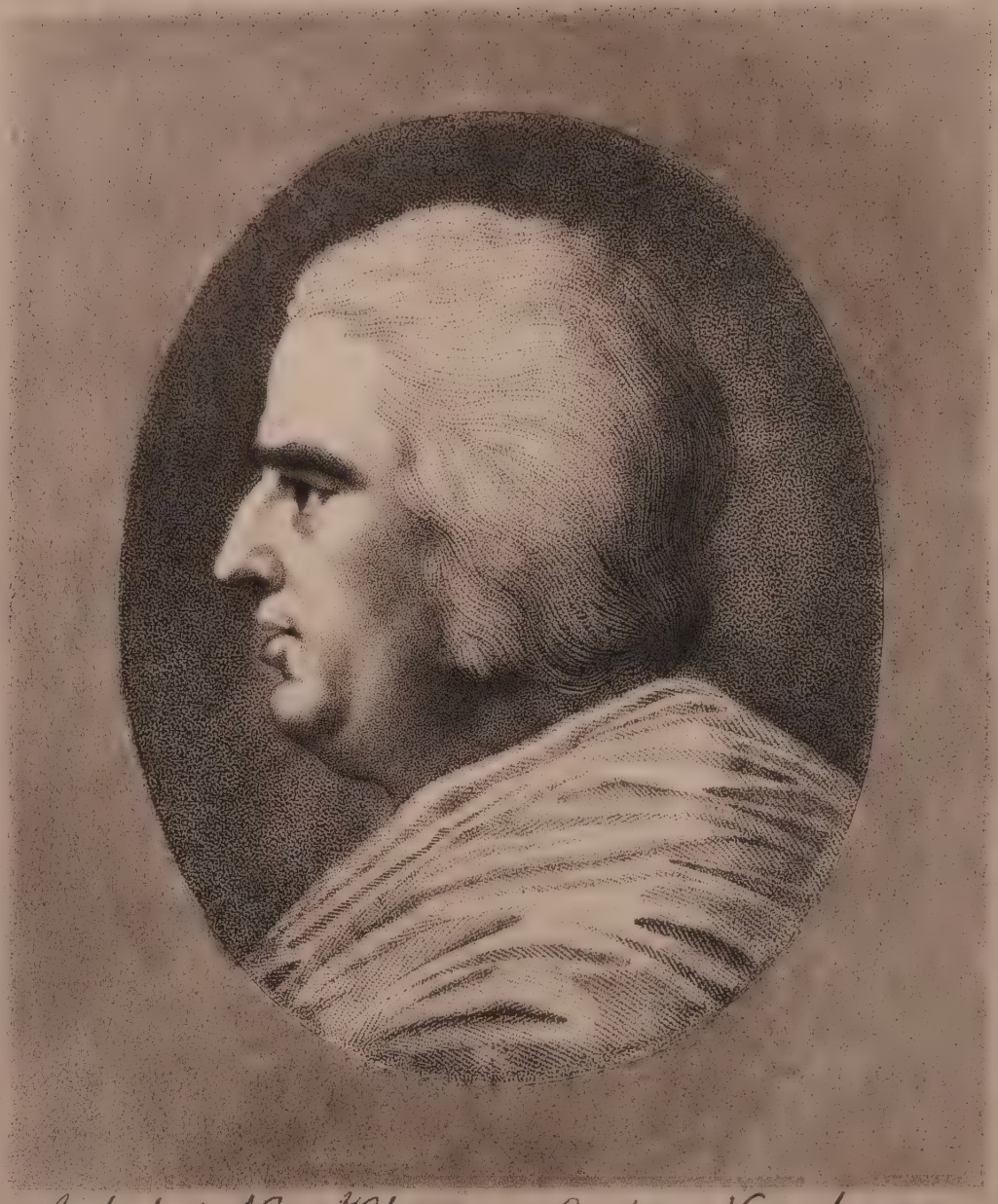
SALINE BLOOD FOR TRANSFUSION.

FROM the fact that the harpooned whale may sometimes live for long periods after profuse loss of blood, owing to the introduction of the saline sea-water into its circulation, I thought it possible that the injection of a saline solution might be effective in sustaining life after loss of blood; and more than once I witnessed in the lower animals apparent reanimation from what seemed actual death. But this only occurred when there was still some remaining circulation, and when some blood was left in the veins with which the saline substances could admix. From thence I drew the conclusion that the saline did its useful work, not by any sustaining power of its own, but by its action in mixing with and forcing blood, still in the vessels, onward in its course. I tried thereupon to produce an artificial saline fluid, like blood itself, that would convey the proper elements and would oxidise in the pulmonary circuit. In this direction I made one good advance. I discovered that if fresh venous blood were added to a saline solution sufficiently charged with proper saline substance to sustain the even fluidity of the blood, such solution, warmed up to natural blood heat, was the best of all injections for restoring the phenomena of life; and I suggested that blood added to a saline solution might be dried down so as to be kept, in sealed bottles, ready for use in transfusion. I believe now it would be most practical to add freshly drawn venous blood to the saline solution at the time it is wanted. A saline solution, consisting of forty grains of chloride of sodium and twenty grains of phosphate of soda in a pint of distilled water, should take up and hold in even solution at blood temperature

ten fluid ounces of fresh venous blood, if the blood, as it flows from the vein, be caught in the solution whilst cold and be gently stirred into it with a clean glass rod. This, I conceive, would be the best of all fluids for venous transfusion in the collapse of cholera and hæmorrhage, in both of which emptiness of the circulatory cavities is one cause of the collapse. In extreme chloroform or methylene collapse, where no blood is lost, a portion of the saline solution alone, with the addition to it of five minims of strong solution of ammonia or a fluid drachm of the aromatic spirit of ammonia to twenty fluid ounces of it, would be a good venous injection in conjunction with artificial respiration.

STYPTIC COLLOID IN ECZEMA.

SHORTLY after I first began to bring styptic colloid into practice, I employed it as an external application in eczema; and I have continued the practice until the present time with every reason for satisfaction. In one instance I painted three parts of the surface of the body of a patient, suffering from subacute eczema, with the colloid, and by that means afforded a relief which every previous application had failed to give. The colloid may be freely applied with a soft brush over the affected surface. It forms a loose scale mingled with the cutaneous scales and exudation; it causes no irritation, and it may be repeated once daily so long as it is required. Applied to the face it lessens rather than increases the disfiguration. It desquamates, so to speak, from the part covered with it, leaving, ultimately, a natural surface beneath without sign of scar or mark.



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JOHN MOORE

M.D.

Autotype from an engraving by Hopwood, after an original drawing.

R.W.L.

JOHN MOORE, M.D., AND "MOORIANA."

THE Englishman of middle age who hears the name of Moore pronounced from a work of history immediately recalls a battle, a hero, and a poem. He thinks of the battle of Corunna, in which Soult was defeated, and the most magnificent retreat ever accomplished by British warlike skill was carried out; he recalls the death of the brave Sir John Moore; but, above all, he remembers the exquisite poem commencing :—

> "Not a drum was heard, not a funeral note,
> As his corse to the ramparts we hurried;
> Not a soldier discharged a farewell shot
> O'er the grave where our hero we buried,"

a poem which alone gave to its gifted and fortunate author, Wolfe, an all-sufficient entrance to the temple of fame.

Strange, with it all, is the fact that the world should have forgotten, and stranger still that the profession of medicine should have forgotten, the fact that the famous hero "left alone in his glory" on the battle-field had a father who in his walk of life was as illustrious as any of his compeers, a man of great and popular mark also, whose name, if it had not been eclipsed by that of his soldier son, would have shone out as permanently as it did shine out temporarily whilst that son was carving his

way to the immortal respect and love of his countrymen all the world over.

Yet this was the fact. Dr. John Moore, whose life I am about to recall to ASCLEPIAD readers, the father of the general, was a man of the most distinguished mark in the professional circles of his day, was the cotemporary of the brothers Hunter, must have come, I suspect, across John Hunter times and often, was the friend and biographer of Smollett, must have known Hewson, and did know many men of eminence so favourably that two of them, at least, the Rev. F. Provost and F. Blagden, put together after his death the leading facts of his career, and under the title *Mooriana* collected many of his best sayings, leaving to us some really valuable history as well as some things of true wisdom, told in a style as enticing as it is worthful.

In the life of Dr. Moore, we have to go a little apart from the strictly practical in medicine, a change for which I make no apology. Medicine is as wide as all knowledge and all wisdom; for does she not comprehend all life, and is not life and the action of life all that belongs to man? It is the glory of Medicine that she is so comprehensive that men of every class—philosophers, poets, naturalists, chemists, astronomers, historians, novelists—have sprung from her, as from the mother of the arts as well as the sciences. Moore, in a sense, was an outsider. After the manner of his friend Smollet the was a traveller, historian, and novelist, yet withal a physician practising as such, at one time working hard in his vocation, and apparently never leaving it until from it death did him part.

By a fortunate circumstance for history, Moore was in France at a time when that country was pregnant of a new political system, after surviving fifteen centuries of monarchy good, bad, and indifferent, and in the end

bad of bad. We have recently been taught by the great work of Felix Rocquain* what was the revolutionary spirit of that astounding period of history. But here was an Englishman who saw the preparation, and who, like Smollett, who also saw it, forecast faithfully the results. This opportunity, and many other opportunities of similar and diverse kinds, rendered Dr. Moore a scholar in general as well as medical learning, and without taking away one iota from his medical accomplishments, in a practical point of view, caused his mind to open to the reception of impressions of knowledge and wisdom until he became, I think, the wisest as well as the best informed of all the physicians of his day and generation. But I must not anticipate. Let the history of the man be the best and truest witness of the fact, and let all inferences stand over until the man is better understood.

BIRTH AND EARLY EDUCATION.

Moore was born at Stirling in the year 1730, two years after the birth of his great cotemporary John Hunter. His father, the Rev. Charles Moore, was a minister of the Episcopal Church of Scotland in Stirling, and was much beloved for the purity of his life, his devotion to his calling, and the benevolence of his nature. His mother, a native of Glasgow, the daughter of John Anderson, Esq., of that city, was a fitting companion for her husband. She was a woman of superior understanding, firm character, and gentle disposition. Unhappily the union of his parents was soon dissolved, for his father died when he was but five years old, and he, an only child, was left on

* Lately condensed and translated into English dress by Miss J. D. Hunting, with a characteristic preface by Professor Huxley.

the hands of his mother entirely for his guidance and support. He went in his early years to the grammar-school in Stirling, and the profession of medicine being that for which he was destined, he was taken by his mother to Glasgow, very early in his life matriculated there, and was afterwards placed with a respectable medical practitioner in the city, Dr. Gordon, to learn the elements of his craft. They must indeed have been elements, for his biographers tell us that in the seventeenth year of his age he left Scotland, and, under the protection of the Duke of Argyll, passed over to the Continent to Flanders, and served as surgeon's mate at the military hospitals there, during the expedition of the Duke of Cumberland. In this capacity, one which was much sought after in those days as a ready means of entry into medical life, he acquired a large amount of practical knowledge and skill, and after two years returned to these islands, and made his way to London. From London, after a brief stay, he went to Paris for the purpose of availing himself of the medical and surgical instruction to be obtained in what was then considered to be the first medical school in the world. He made a fortunate start in Paris. The British ambassador at that time was Lord Albemarle, a nobleman who had acted as general under the Duke of Cumberland, and who had also been colonel of the Coldstream Guards, to which regiment Moore had acted as surgeon's mate. The ambassador received the young surgeon, when he paid his respects at the Embassy, with great favour, took him under his protection, and appointed him surgeon to the Embassy, as well as to his own family. He therefore remained in Paris two years, in practice on his own account in English circles, and paying regular attendance at the hospitals and courses of medicine that were open to him for study.

PROFESSIONAL CAREER.

At the end of these two years, Moore returned to Glasgow, graduated there, joined his former master or tutor, Dr. Gordon, at his request, as a partner in practice, and about 1769 married Miss Simpson, the daughter of the Professor of Divinity in the University of Glasgow, and by her had five sons and one daughter, his first-born becoming the famous Sir John, the hero of Corunna.

The partnership with Gordon was mutually advantageous, and until he attained his fortieth year Moore remained steadily at work as a physician in Glasgow. During this time he became the favourite medical attendant of the Hamilton family, giving the most conscientious care to the case of James George, Duke of Hamilton, a youth who after a lingering consumption died in his fifteenth year.

The attention of Moore and his real affection for his patient endeared him to the family, and when Douglas, the brother of the late Duke and successor to the title, himself in very delicate health, was obliged to go abroad on travel for the sake of his health, Moore accompanied him for a period of five years, namely from 1772 to 1777. In the course of this time, he travelled through France, Italy, Germany, and Switzerland, making the most careful observations of men and things, forecasting events, and preparing notes for books of travel which were ultimately to become amongst the best-known works of the time, one of them passing through nine editions. His connection with the Duke of Hamilton, all through, was of the greatest service to him in his studies. It permitted him to enter into what was considered the highest classes of society, and it did not prevent him from going amongst the lowest, in order to glean facts and figures on all sides.

We shall see when we come to consider the literary work of Dr. Moore that his residence in France and his travels in other parts of the Continent gave him insights into then current events which were well and faithfully utilised. It is not wrong to say that he enjoyed his exile, but on coming back to this country he determined to settle himself for the remainder of his life in the great metropolis. He returned in the year 1778, took up his residence with his family in London, and entered into practice, after the manner of a general family physician; unconnected with the Royal College of Physicians and with any special branch of practice. It is said that he did not enrich himself on the labours of practice, and from the prolific nature of his literary work it is certain he had not much time on his hands to attend very closely to the sick and afflicted. He took whatever practice came to his hand, without much trouble or searching after it, and let his active mind revel in the pleasures of imagination and the pursuits of learning, the friend of all men of culture, the close friend of liberty, and, in his circle, an authority much respected and warmly admired.

In this life of home delights, in which his wife, his five sons, and one daughter, heartily shared; esteemed by "a numerous public as a judicious and prudent physician, who, considering himself an assistant to Nature, left her when her power seemed sufficient to perform her own cure and assisted her when too feeble by every means in his power"; in this happy life, with the beautiful garden of literature ever under cultivation, he lived on until the year 1792, when his career was suddenly diversified by another call to Paris, in company with his old friend Lord Lauderdale,—whose state of health demanded the care of a wise physician,—and with one of his own sons. They embarked at Dover on the 4th of August, 1792, and in a few

hours arrived at Calais. Next day they reached Abbeville, where, although the descent of an Austro-Prussian army was expected, they found the country people enjoying themselves in "dancing cotillons under the shade of trees," as if all were peace and pleasure. On the 6th of August they reached Clermont, meeting many flying people, and on the 7th they were in Paris.

At the moment when Dr. Moore found himself in the French capital, that capital was in the height of political fire. He made his way to the Assembly to listen to the fire of the debates. At two o'clock of the morn of August 10th, he was awakened by the sound of the tocsin in the streets, and at nine by the firing of the cannon which preceded the downfall of a monarchy that for so many centuries had governed, without serious dispute, the destinies of France. Soon after the firing had commenced, he heard the cry of "To arms, citizens, to arms! They slaughter your parents, your brethren, your sons," and he saw men running half frantic through the streets exclaiming in that manner. Lord Lauderdale was too unwell to quit the house, the Hotel de Moscovie, in the Faubourg St. Germain, but Moore, leaving his son with the invalid, went out to witness the scenes that were in such fearful progress. A number of armed citizens were marching towards the Tuileries; another set followed dragging five cannon along the Quai de Mazarin, where he waited, to the Pont Royal. Here he ran a great risk, for as a body of pikemen passed him one of them pointed him out as an aristocrat, a fatal distinction. Fortunately the "*valet de place*" who had accompanied him called out that, so far from his being an aristocrat, he was "*un Anglais.*" "*Bon!*" cried the pikemen, and marched on.

These were critical hours in the seat of revolution, and the reader will feel no surprise when he is told that

Dr. Moore and his friends were not slow, as matters progressed from bad to worse, to leave a city in which danger as well as horror marched, hand in hand, daily. They left Paris on September 4th, Lord Gower having been recalled from the Embassy on August 28th, and arrived at Chantilly the same day. On the 5th they reached Amiens without any misadventure, and there found the statue of the great Condé, which stood in the grand staircase of the castle, bereft of its head. "I never," said Moore to the attendant who showed them the apartments of the castle, "have seen Chantilly *dans un si triste état.*" "*Aussi, Monsieur,*" answered he with a sigh, "*n'a-t-il jamais été si triste.*" Travelling through Flexcourt, they came on to Abbeville and so to Boulogne, which last city they reached on September 10th, and remaining there a day or two, passed on to Calais, where they stayed until the beginning of October. Then, having received information that a National Convention from all departments of France was assembled in Paris, and that the Prussian army had retreated, it might, they imagined, be the fact that a permanent and free government would be established. It was not a very hopeful look-out from the accounts given of some of the deputies; yet, as on the measures which this assembly should adopt the fate of France, perhaps the fate of Europe, might depend, Moore and his friends resolved to set out again for the capital, by the way of Lille, in order to be eye-witnesses of the proceedings that were to take place. They reached Paris on October 10th. They remained there until December 14th, after which they were not long before they returned to London, where Moore resumed his labours in practice and literature, with plenty of occupation on hand from both sources.

THE WORKS OF DR. MOORE, MEDICAL AND GENERAL.

Moore was in the truest sense a many-sided as well as an accomplished and learned man. If I said of him that he had true genius I should not speak faithfully. He was deficient in the poetic faculty. He was laborious, painstaking, broad, and liberal in thought as well as action. He had the gift of imagination, but that gift was either limited by nature, or his practical soul made him timorous when his imagination wished to take wing. This is poor progress, and has spoiled many a great man. Imagination is not given to a man to be cribbed and confined like a prisoner, and held subservient to what this pedant will think or that prig will say. A moderately endowed imagination may manage to live; a cowardly imagination may for a moment lift its head; but the imaginative genius that is to float over the ages, and be known as a star in the firmament of fame, even though it be as a star of smallest magnitude, must be free as the eternal space in which it moves, and lustrous, however faintly, of eternal fire. Moore was no coward; he was as brave a man as ever Scotland produced, and as honest; he had imagination with courage and honesty; but the imagination was by its own nature limited, and therefore not such a light to the ages as to hold the attention of mankind by its own intensity. Its light was diffuse; it was medical, it was philosophical, it was imaginative, it was historical; and it displayed itself in each of these directions. We will contemplate it in the divisions so specified.

Medical Works.

In the year 1785 Moore published a treatise entitled *Medical Sketches*. It was what one might suppose it to

be from its name. It was devoid of all technical terms and technicalities; was addressed, in fact, to the educated few of all professions, rather than to the one profession. Sometimes there is in it a touch of humour, which is, however, too guarded and delicate to be of remembrance as true and lasting humour. Medically, there is about the work much compilation, with a wisely running commentary showing a well-read man in medicine, and maybe a full man, without betraying a searching man, or an original. Anecdote abounds, so that the volume is rather better fitted for the armchair, the bed,—just before dropping off to sleep,—than for the study. No one asking for more light on a troublesome or serious case of disease would take it down for the purpose of getting light out of it; but it is good of its kind, and is of profit for leisure. The "*Mooriana*" of his biographers supply, very happily, many of his views respecting things medical. He complains of the ignorance of physicians, especially in reference to the nervous system. "If," he says, "the ablest mechanic were to attempt to remedy the irregular movements of a watch while he remained ignorant of the structure and manner of acting of some of the principal springs, would he not be in danger of doing harm instead of good? Physicians are in the situation of such a mechanic; for although it is evident that the nerves are the organs of motion and sensation, yet their structure is not known. We are ignorant why the will which has no influence over the motion of an animal's heart should find the feet obedient to her dictates; and we can no more explain how a man can move one leg over the other by volition, or the mere act of willing, than how he could by the same means move Ossa on the Olympus. The one happens every moment; the other would be considered a miracle. While so many parts so infinitely essential to

human life are not understood, instead of being surprised that so many diseases baffle the skill of the physician, we have more reason to be astounded that any can be alleviated or cured by his art."

Referring, in another place, to the brain, he remarks: "The sum of our knowledge upon it is, that the brain is the seat of thought and origin of all sensation and motion, that every sensation is conveyed thither, every idea formed there, and that the power of every motion proceeds from thence through the nerves to the muscular parts of the body."

The practice of physic he held to be that of all others in which the generality of mankind have the fewest lights by which they can discern the abilities of its professors, because the studies which lead to it are "more out of the road of usual education, and the practice more enveloped in technical terms and hieroglyphical signs." But the safest criterion by which men, who have not been bred to the profession, can form a judgment of those who have, is "the degree of sagacity and penetration they discover on subjects equally open to mankind in general, and which ought to be understood by all who live in society."

Valetudinarianism.

Occasionally he follows Molière in his humour on this topic. He relates an anecdote in which a valetudinarian and his physician are in consultation. The patient is assured that he is physically in good condition; then observes the patient, "What a pity it is that I never had the gout! That is a disease, I understand, which removes others that have resisted every method of cure." The physician assents. "Cannot you then give me a fit directly?" asks the patient. "Indeed I cannot," the physician responds. "Yours seems to be a very unfortunate profession, doctor,"

is the retort; "for although you deal entirely in diseases, yet you are neither certain of removing them from those who have them, nor of giving them to those who have them not."

"What you observe is very true."

"A good many of your profession have had the honour of knighthood conferred on them of late."

"A great many, sir; by-and-by I suppose no man will presume to practise medicine without it."

"As in the days of chivalry, when no man could kill on the highway till he was dubbed a knight. But what do you intend, doctor, to prescribe for my complaint? You must be sensible that there is no time to be lost."

"True, sir; I shall order you some restorative draughts; but I must assure you, at the same time, that their good effect will be greatly assisted by your riding on horseback three or four hours every day before dinner."

"You mean when the weather is fine?"

"I mean in all weathers."

Aura Epileptica.

He paints very clearly the phenomenon of the "*aura* in epilepsy," described by some as like to a current of fresh air, flowing slowly; by others like an insect moving or creeping upwards from the extremities of the body to the head, where it no sooner arrives than the patient becomes insensible, and falls into an epileptic fit. This first sensation Moore thought is generally felt at one particular point, and we may presume there is some disorder in that point from the circumstance of the *aura* beginning there. The disease, he supposed, might be radically cured by destroying the part; when this cannot safely be done, the fit might, he assumed, be prevented by a ligature applied above the part where the *aura* arises.

Views on Hypochondriasis.

He describes hypochondriasis as a disease arising often in persons of previous good health, and in opulent circumstances; and he seems to accept a common opinion in his day that it is a malady singularly frequent in England. The patient, he explains, becomes regardless of things of the greatest importance, and which formerly interested him most; he is averse to every kind of exertion of body or mind; he becomes infinitely attentive to a thousand trifles which he used to disregard, and he is particularly watchful of every bodily feeling, the most transient of which he often considers as the harbinger of disease. Everything appears to him dark and dismal. He is always apprehensive of the worst, and considers the most indifferent and even the most fortunate incident as the omen of some impending evil. Although in his brighter days he may have been a man of courage, he is now preposterously afraid of death, although he may seem to have lost all relish for the enjoyments of life. The wretched creature, growing every hour more irritable and peevish, flies at length to quacks. Their well-attested and infallible remedies hurry on the bad symptoms with double rapidity; he returns to physicians, goes back to quacks, and occasionally tries the family nostrums of many an old lady. His constitution being worn by fretfulness and by drugs, he, at length, despairs of relief, and either sinks into a fixed melancholy, or, roused by indignation, his good genius having whispered in his ear, "*fuge medicos, et medicamina*," he abandons the seat of his disappointments; tries to dissipate his misery by new objects and a different climate; consults no practitioners of any country, sect, or denomination; and forms a fixed resolution to swallow no more drugs, from which happy

epoch, if the case be not quite desperate, he has the best chance of dating his recovery.

Power of Curing Diseases.

On the question of the power of curing diseases possessed by medical men, Moore thought that, in accordance with the experience of ages, the great and ultimate effect of the art of medicine, cure, is more effectively attained by diligently observing the course and symptoms of distempers, and the effects of the means used for their relief, than by the most plausible reasonings on their supposed nature and causes. This purely empirical argument of Moore was supported by a theory which all would not agree with; namely, that the plan of seeking into causes has proved more attractive to many ingenious men of the profession, for a reason sufficiently obvious, to wit, "that it suits, in a particular manner, such as are employed in lecturing to students of medicine."

Remedies for the Influenza.

In the time when Moore lived, influenza, as in our time, was sometimes epidemical, and many remedies were then, as now, propounded for it. Our rather sceptical doctor—for he was a little questionable on some important points of ancient orthodoxy—indicated his faith in remedies by the following dialogue:—

"A country apothecary in the course of conversation told a gentleman that he had been visiting three or four patients in a neighbouring village who were ill of an influenza.

"'Pray,' said the gentleman, 'how do you medical gentlemen treat that complaint?'

"'Why,' replied the apothecary, 'there is no saying how the physicians treat it, for some of them order one thing

and some of them another; but the surgeons generally bleed for it; and as for us apothecaries—we *drench*.'

"'Ay! that is natural enough,' said the gentleman; 'but you know I once studied physic myself, and have some general notions on the subject; so when this epidemic began amongst my servants and tenants, I ventured to prescribe barley water for them.'

"'Barley water!' cried the apothecary, with disdain. 'You might as well have ordered them water gruel!'

"'I might so, for which reason I gave them the choice, and what do you think was the effect?'

"'Why, barley water and water gruel could have no effect,' said the apothecary.

"'It is indeed difficult to ascertain what is the effect of many things that are prescribed; but I can at least tell you what was the consequence.'

"'That is the same thing,' said the apothecary.

"'Not quite,' replied the gentleman, 'though in the practice of physic one is often mistaken for the other.'

"'Well, what was the consequence?'

"'Why, they all recovered!'

"The apothecary having remarked that there was a great difference in constitutions, abruptly took his leave."

Vis Medicatrix Naturæ.

Moore describes the vis medicatrix as "that inherent bias in the animal economy leading to restoration of health from disease." He illustrates it by a simile: "As the surface of a lake which clearly reflects the sky, the hills, and the verdant scenes around its borders, when it is disturbed by the falling of a stone, immediately endeavours to recover its scattered images and restore them to the same beauteous order in which they were wont to appear, so in like manner, when the natural course of the

animal economy is interrupted and disturbed by disease, the powers of the constitution are continually endeavouring to restore its organs to the perfect use of their functions and to recover its usual vigour and serenity."

"The vis medicatrix," he adds, "was observed by the father of medicine." The same observation was expressed by Sydenham: "It is acknowledged by all candid and discerning practitioners to have a powerful influence in the cure of diseases. Indeed, physicians, in proportion to their candour and discernment, acknowledge and rely on this power of nature; and in proportion to their selfishness and weakness impute every recovery to their prescriptions."

Medical Errors of the Fifteenth and Sixteenth Centuries.

The gloom which seems to have settled on the minds of men in the fifteenth and sixteenth centuries was a theme on which Moore has two or three times commented. In religion and in medicine everything pleasing was thought to be sinful, and by many "enthusiasts what gave the greatest pleasure was considered the greatest sin." The physicians applied this idea to the practice of medicine; they condemned everything that was most agreeable to a sick person's tastes or feelings, declared it noxious to his constitution and the more noxious in proportion as it was agreeable. In many instances they treated their patients as if they had been persuaded that the most effectual way to restore health was to prescribe what was most repugnant to the taste of the sick man. If he complained of heat, additional bedclothes were heaped upon him to force a sweat; if, half stifled, he begged for a little fresh air, the bed-curtains were drawn closer, because cold ought to be most guarded against when the body is hottest; and if he complained of thirst, and en-

treated for a draught of cold water, he was presented with a draught from an apothecary's shop well impregnated with spiceries. "How many victims," he asks, "since the period alluded to have been sacrificed to the pride and obstinacy of mistaken science, dazzled by the meteors of theory, and despising the humble paths of experience pointed out by the earlier professors of the healing art!"

The Concluding Scene of Nature.

Hippocrates himself might almost envy the graphic description which Moore has left of the last hour of life in man. Of course the description does not apply to all last hours, but it does to many, and it may be accepted as a fair picture of the natural and final collapse which follows gradual asthenia from acute disease and from decay of vital power :—

"When nature and the efforts of the physician prove unable to resist the malignity of disease, all the distinguishing marks of disease are obliterated, and the concluding scene is common to all.

"The strength being almost entirely exhausted, the patient lies constantly on his back, with a perpetual propensity to slide towards the bottom of the bed; the hands shake when they attempt to lay hold of anything, and a continual twitching is observed in the tendons of the wrist; the tongue trembles when it is pushed forth for inspection; or all attempts to push it forth are unsuccessful; a black and glutinous crust gathers on the lips and teeth, to the increase and inconveniency of which the patient seems now insensible. He seems equally insensible to the ardour of thirst; he mutters to himself; he dozes with his mouth half open, the lower jaw falling down as if the muscles were too much relaxed to resist its gravity; he sees objects indistinctly, as if a dark cloud hung before his

eyes; small black particles, called by the physicians muscæ volitantes, play, as it is believed, before his eyes, for he often catches with his hands at those or some other objects of his disordered brain; he frequently extends his arm before and above his face, seeming to contemplate his nails and fingers, and picks the wool from off the bed-clothes; he loses the power of retention; the evacuations pass involuntarily; and as if lamenting his own deplorable condition, tears flow down his ghastly countenance; the pulse flutters small as a thread, and on a pressure very little stronger than common is not felt at all; his legs and arms become cold, his nails and fingers blackish; his respiration is interrupted by hiccups and finally by death."

Temperance and Health.

Had Moore lived and flourished in these days of progressive temperance, he would have been welcomed amongst the abstaining ranks as an invaluable ally. He was far in advance of his time on this subject. Speaking of intoxication, he reckoned it amongst the causes of fever and declared that the general bad effect of it upon the constitution is obvious.

"Whenever," he says, "a predisposition to any particular disease lurks in the constitution, intemperance in drinking seldom fails to rouse it into action. Repeated excesses of this kind sometimes produce the epilepsy in those never before subject to it, and always hasten the return and augment the violence of the fits in those who are.

"To increase good humour, gaiety, and wit, and prolong the pleasures of conversation, is the usual apology for such excesses. But if it were a general rule to leave the company as soon as our taste and talents for sensible or witty conversation began to diminish, few would injure their constitutions by drinking.

"There are, indeed, examples of people who support long and repeated excesses without much apparent injury. There are also instances of people who have swallowed poison with impunity. But tell those who are acquainted with such tough and well-seasoned veterans to recall to memory the numbers of their companions who, yielding to importunity, have fallen victims to this easiness of temper, and they will not be much encouraged by the example.

"Although intoxication never fails when first indulged to produce most of the symptoms which attend fever, as heat, drought, headache, and nausea, it must be confessed that these wear away by habit, so that those who indulge every day in the bottle, if they survive the excesses of their youth and escape consumptions, dropsies, and paralytic complaints in more advanced life, are in little danger of being cut off suddenly by a fever from drinking; they will have the comfort of outliving not only their friends, but very probably their own understandings.

"In some instances where people have fallen down insensible by extraordinary excess in drinking, a supervening fever has been considered as the only thing that saved them from a fatal apoplexy. It must be allowed that a disease must be of a very desperate nature for which a fever is the only remedy, and this remedy not always effectual; for it sometimes happens, particularly to young persons of a sanguine habit, that, in consequence of great excess in drinking, a fever of such violence is raised, that the patient dies after a few days of high delirium." *

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* "Hippocrates mentions in his third book of Epidemics the instances of two persons who—ex potibus ambo periculosa febre decubuerunt; quorum primus secundo jam morbi die surdus factus fuit, dein ferociter deliravit, quarto die convulsus, quinto die periit alter vero, post difficilem morbum, vigesimo die evasit."

In another characteristic passage Moore points out how the action of alcohol tends to promote ennui and depression of the mind. The passage is very true and striking:—

"Of all the contrivances to exclude this intruding demon, *ennui*, from the mind of man the most debasing and destructive is the use of intoxicating liquors; that pernicious habit blunts all desire of improvement, darkens emulation, obscures the understanding, sinks the soul into sluggishness, renders men insensible to the love of reputation, familiarises them with the idea of contempt, and extinguishes every enjoyment but that maudlin delirium, excited by spirituous liquors, which carries them to their graves."

Works Philosophical and Imaginative.

The philosophy of Dr. Moore is shown rather in scattered passages of his writings, than in the compass of any one essay or volume. As a rule it is always sound and pleasing rather than abstruse and puzzling. It consists, usually, of short passages on different topics, suggested by sayings or events, not from systematised order of thought. Thus, on the words of Apelles spoken to the cobbler *Ne sutor ultra crepidam* he contends that the maxim applies to such professions as call for mechanical skill only, not to exertions of the mind. A man, however ingenious, who would attempt to make a chair without being bred to the work would certainly succeed much worse than one accustomed to the trade; but the mind is more flexible than the muscles, and the man who has been in the habit of reflecting and reasoning all his life will reason better, even on a new subject, than another man of a narrower understanding who has been accustomed to investigate it. In example, he shows that men who have not been trained in the art of war and men who

have not been bred to statesmanship have often greatly excelled in those arts, and been rendered famous by the display of talents which had previously laid latent in them. He makes many sensible observations on the passions of mankind; explains that hatred always betrays itself, that seduction is a very feeble justification of a *faux pas*, that every sentiment of the mind has particular parts of the body in correspondence with it, and that first impressions, which sink into the heart and form the character, never change. He held that ideas were not innate, and adduced in support of that belief the saying of a young Oxonian, who, being told by his tutor that the most permanent happiness of life proceeded from our ideas which were not innate, replied, "I am sorry for it, for if they had been we should not be put to the trouble of reading for them."

The imaginative works of Moore were three in number. The first was a story called *Zeluco*. It was published in 1787, and was presumed to be educational and moral in its meaning. It is, in so many words, the life of a rich rascal who, spoiled by his mother and trained to no self-control, became about as infamously wicked as it is possible for anything human to become anywhere. The author says that in this story he was "tracing the windings of vice and delineating the disgusting features of villainy." The story must have attracted, for the *Childe Harold* of Byron was a poetical *Zeluco*. This novel was followed by one called *Edward*, 1790, the hero a kind of second Tom Jones, less the fine passages which introduce each chapter of that mortal immortal fiction. It introduces a heroine in the character of a truly good woman whose worth surpasses and supplements admirably the absence both of beauty and accomplishment. The third story, styled *Mordaunt*, was constructed after the manner of Samuel Richardson

in that it takes the form of a series of letters. In these the life of the principal person is described by himself to a military friend named Colonel Sommers. The work introduces "*Sketches of Life, Character, and Manners in Various Countries, including Memoirs of a French Lady of Quality.*" It was published in the year 1800, and was considered, in consequence of its anecdotes and incidents of travel, the best work of fiction that Moore produced.

Historical Works.

It remains for me now to dwell for a brief period on the character of Dr. Moore as the historian. In this position he holds a very distinguished place, a place that has been but poorly recognised in the world of literature. In his historical labours he gave little vent to imagination, made comparatively few commentaries, spread out no long-strung details, but kept to his subject and related, most carefully, that of which he was the eye-witness. The best of his works, the most forgotten, and, at the same time, the most likely to be enduring, is that which records what he witnessed in the furiously fiery days of the great French Revolution of 1792. He went to France at that time; stayed in Paris until after the eventful 10th of August; retired to Calais; returned to the capital after the appointment of the National Assembly, and remained there until a few weeks before the execution of the King, collecting his observations and experiences. These formed two volumes published as a *Journal* by Robinson, of Paternoster Row, in the year 1793. They bring their author's personal knowledge up to the 14th of December, 1792, and they contain a statement, derived from the best sources, of the events which culminated in the execution of the King on the 21st of the next month, January, 1793. It shows

an astonishing courage on the part of Moore that he ventured to wander about as he did in Paris in the midst of all the prevailing horrors of the time, and entered into conversation with the most excited revolutionists in order to obtain a faithful record of events and expressions of opinion. In the club of the Jacobins, in the National Assembly, in the streets of the distracted city, in the shops and hotels, amongst the rich and amongst the poor, there he was witnessing scenes from which he was sometimes obliged to turn away, but never indicating the least fear on his own account. He watched the prime actors in the awful tragedies that were being played, and depicted some of them with a masterly hand. He knew Marat as one of the vilest and cruellest medical lunatics medicine ever had in her ranks, and described the ferocious monster in words that almost burn as they are read:—

MARAT.

"Marat is a little man of a cadaverous complexion, and a countenance exceedingly expressive of his disposition. To a painter of massacres, Marat's head would be inestimable. Such heads are rare in England, yet they are sometimes to be met with in the Old Bailey. The only artifice which he uses in favour of his looks is that of wearing a round hat so far pulled down before us as to hide a great part of his countenance. The man's audacity is equal to anything; when he is in the tribune he holds his head as high as he can, and endeavours to assume an air of dignity. He cannot succeed in that, but amidst the exclamations and signs of disgust the look of approbation which he wears is wonderful: far from ever having the appearance of fear or of deference, he seems always to contemplate the Assembly, from the tribune, either with the eye of menace or contempt. He speaks in a hollow

croaking voice, which, in such a diminutive figure, would almost produce laughter were it not suppressed by a horror at the character and sentiments of the man. When he attempts pleasantry it increases the horror which his appearance creates; it gives something of the sensation which I imagine I should have if a murderer, after cutting a man's throat by a dexterous stroke of a knife, should smile in my face and tip me the wink. Marat is shunned and apparently detested by everybody. When he enters the hall of the Assembly, he is avoided on all sides, and when he seats himself, those near him generally rise and change places. But nothing can disconcert him. I have never heard of any other of his good qualities; but he certainly possesses a great deal of courage, both personal and political: no danger can terrify him; his heart as well as his forehead seems to be of brass."

Is there a finer or more graphic picture in history than this which Dr. Moore drew from personal observation of a little villain, who, in spite of all, was a favourite with many, and who even gained a kind of blind affection as one who was so clever that his openly denounced misdeeds were glossed over by the cleverness with which they were evaded? "*Il était un petit diable en pouvoir*" was truly said of him.

A picture of Mirabeau is admirable; but Robespierre, by the strange contradictions of his nature, was a madman Moore does not seem to have comprehended. The picture is true as far as it goes, but lacks completeness and perspicuity.

Taking it all in all, the *Journal* of Moore is the best account I have read of the great Revolution. I believe I have read all the choicest histories of it, and I felt as I waded through Carlyle, missing no line and construing the meaning line by line, that his story was complete,

and that nothing could be more vivid from the dead past. But this much simpler story of Moore has in it truer life and carries the mind and heart nearer to the dreadful reality. It is the narration of one who was there, who guesses nothing he does not know, and, content to tell all he knows, tells no more. When he gives reasons for the Revolution they are not speculations; they are the reasons delivered to himself personally by those who were playing an active part in the astounding commotion. The temptation is strong to follow up further the character of our scholar as historian, but the task must end in this epitome.

The literary style of Moore is simple and clear, never wearisome and always enticing. He was a man of letters by nature, and his *Smollett* holds its own as firmly as the works of that illustrious author, about whom I shall have soon to write a word. If the time were at my command, and the means, I should feel it a delightful labour to republish the whole of the works of Moore in a cheap and popular form. They would, I am sure, be largely read at this particular time, both at home and abroad; for though it be true that the times change and we change with them, there is in his pages many a lesson that would be singularly appropriate at the present hour.

Final.

After his return to London, Dr. Moore kept the even tenor of his way, occupied steadily in his literary and professional pursuits. He had five sons and one daughter, the welfare of whom was his ever constant care. They well repaid his affection. John, the eldest, after distinguished service in Corsica and in Egypt, and after winning the love of all whom he with intrepid courage and skill commanded, died, as we know, a brave soldier in the

hour of victory, the happiest death such a man can have.* James became a surgeon of considerable celebrity in London. He added to the literature of medicine, and proposed that new method of producing local anæsthesia, by pressure on nerves, described in the Life of Benjamin Bell in ASCLEPIAD 26, vol. vii., p. 355. Graham entered the navy and distinguished himself when, in command of H.M. ship *Melampus*, he took the *Ambuscade*, one of the squadron intended for the invasion of Ireland. Francis entered the diplomatic service, winning laurels there. Charles, the youngest son, won for himself distinction at the bar. Miss Moore remained single up to the time of the death of her father, an event which took place, after a lingering illness, at Richmond in the seventy-first year of his age, on January the 21st, 1802.

Dr. Moore, say the friendly biographers who knew him during his life, "paid with resignation his debt to nature, leaving as example a life of integrity, and sons heirs of his virtues, who will, like him, consecrate them to the welfare of their country."

Never was commendation of personal merit more truthfully expressed, never prophecy more faithfully fulfilled.

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* My friend Mr. J. C. Roger, F.S.A.S., to whom I am indebted for many valuable suggestions in regard to Dr. Moore, directs my attention to a severe and very just censure in the *Edinburgh Review* on the famous author of "*Don Roderick*" for "grudging," on political grounds, a single ray of glory "to gild the name" of this illustrious soldier. It was, indeed, very little of so great a writer as Sir Walter Scott to be guilty of so serious an omission.

*THE CEREBRO-SPINAL AXIS AS A THERMAL CENTRE AND WATER POWER.**

MR. PRESIDENT AND GENTLEMEN,—

Whilst I feel very much honoured by the presence here to-day of the members of the *Medico-Psychological Association of Great Britain and Ireland* to listen to a theory from me on the *Cerebro-Spinal System as a Thermal Centre and Water Power*, I am, I confess, not a little anxious respecting the result of my labour. I know I stand before a body of listeners who, from their daily avocation as well as from their accomplished training in mental and physical science, form naturally the most critical audience I could address on the subject to be discussed, and this alone is a cause of grave concern. But there is something more before me. I am about to bring forward an entirely new line of research and observation in so elementary a stage, that the simplicity of the thesis itself may, at first sight, seem to imperil its acceptance. I must, therefore, with special emphasis, claim, at the onset, your kindest indulgence.

Profound researches have been made into the anatomy of the cerebro-spinal system, into its microscopical structure, into its chemistry. Endless experiments have been

* Lecture delivered to the members of the Medico-Psychological Association at a meeting of the Association held at 25, Manchester Square, on Thursday, November 19th, 1891, E. B. Whitecombe, Esq., President, in the chair.

performed in order to discover the functions of its different parts; but I am aware of nothing that has been done towards the study of it as a working mechanism, as a fixed central physical instrument playing its part methodically, according to the physical conditions in which it is placed, apart from the more refined details of function which its delicate and minute structures adapt it to perform in many and marvellous ways.

Preliminary Note.

Before I come to the new work on which I wish particularly to speak to-day, it is necessary to refer to the lines of thought and experiment that have led up to the present labour. Experiment-making is a very slow and suggestive process. One new observation often unexpectedly opens up another. Constantly the route towards a certain end which, at first, looks clear enough, is found to terminate in a *cul de sac*. Very often it is necessary to go back altogether. Occasionally a new and promising path is disclosed, and all ends well. I hope, almost against hope, it is so now.

ELECTRICAL RESEARCHES.

Early in my physiological career I sedulously enquired, by experiment, whether the nervous centres could be charged with electric energy. I tried to construct out of the animal structures a Leyden jar, and to discharge it in regulated directions by conductors. Some curious experiments were arrived at in this enquiry, by which I learned after a certain fashion to charge and discharge from the dead brain. But here came the practical difficulty that to do this some foreign structure had to be introduced into the experiment. To explain more precisely,

I could not find any structure or tissue of the body that would serve effectively as an insulating medium. All parts were conductors to some extent.

In these labours I had the invaluable aid and advice of the late Mr. Becker, one of the best mechanical electricians of his day; and he agreed with me that in the animal organism there is no insulation. He constructed for me a battery and resistance measurer, by which I carried on some experiments of dynamic order; but the results were uncertain and unsatisfactory.

THEORY OF A NERVOUS ETHER.

In the end, I came to the conclusion that some other more definite principle must be sought after than what is now called electrical energy in the nervous centres, and it then entered into my mind that there might exist in the nervous matter a refined ethereal body, to which I gave the hypothetical name of *nervous ether*. I devoted many months to the study of this subject, and published the description of my views in the *Medical Times and Gazette* for May 6th, 1871. The idea here suggested had much on its side, and many persons of mark looked upon it with favour. It was hypothetical, and yet it had many strong points. I supposed that the assumed gaseous or vaporous substance called ether was a chemical product of low boiling point, diffused through the water of the nervous matter; that at the temperature of the blood it was at considerable tension; that it was easily condensable by cold; that it was soluble in water; that in its gaseous or vaporous state it was a medium by which all vibrations were received and conveyed by the organs of sense from without to the brain; and that the collapse of death was due to the cessation of its production, its condensation

and inertia. The field of enquiry in the study of this hypothesis was one of the most laborious I ever trod, and was only relieved of its weariness by the fascination of the pursuit. I tried the absorbing power of the brain for every light chemical body that was likely to answer the probable requirements of a nervous ether. As a result I obtained some curious and useful facts, but I failed in the end to satisfy myself of the existence in the brain of an ethereal substance that would serve the purposes named.

RESEARCHES WITH EXTREME COLD GENERALLY AND LOCALLY APPLIED.

The next line of research was of a different stamp, more practical, and yet broadly suggestive. I made a practical study, as you know, of the common ethers for the production of local anæsthesia by extreme cold. At first this study was applied to the effects of cold on the outer surfaces of the body, and to the sensitive terminations or peripheries of nerves. Here it succeeded well, and I began to extend the study to central nervous matter with remarkable and unexpected results. By subjecting the cerebral mass to such a degree of cold that it underwent congelation, all the voluntary functions of the body were suspended. Precisely as in hybernation, an animal whose cerebral centres were subjected to cold lay in deep sleep, the respiration and the circulation proceeding as before, and sustaining the life. If the process of chilling the cerebral substance were rapidly produced the spinal cord was rendered irritable, and unconscious muscular movements were for a time produced; but if the process were conducted while the rest of the body was exposed, at the same time, to moderate cold, so that the break of function between the brain, medulla, and cord was not too abrupt,

the torpor was unattended by extra movement of muscles. In the torpor all communications between the external world and the animal were cut off. In plain terms the cerebrum ceased, for the time, to be an absorbing centre. The great centre of the volition, cold and consolidated, would not receive light, would not receive sound, would not respond to pungent vapours. It was in the same dead condition as my own skin frozen at a limited point; it would not receive the impression made through a nerve; it was dead to common sensibility, not to mention pain; it ceased to be able to accept or reflect any vibration whatsoever. The water in its substance was waveless, and for the moment dead.

It was remarkable to observe that, although the resistance to absorption of vibrations was so complete that actual death could not have intensified it, there was, if the process were skilfully carried out, no death. The vital acts of circulation and respiration were still in progress, and if the nervous structure were allowed to return to its natural state in so gradual a manner as not to cause abrupt change of structure and injury, the recovery from the dead state was a sure and harmless as well as a painless restoration. In the case of warm-blooded animals, like birds, the artificial hybernation could be maintained for many hours with complete recovery if the temperature were allowed to return, slowly, to the natural state. In the case of cold-blooded animals, frogs, toads, and fish, the brain and spinal cord could be brought to inertia, and held in that condition for much longer periods. In my Croonian lecture to the Royal Society on *Muscular Irritability after Systemic Death*, I showed batrachians enclosed in ice, and exhibited their recovery to perfect life from that extreme condition. In another lecture I exhibited some carp that had been accidentally

frozen in the Zoological Gardens during a hard frost. They were frozen so completely through that they were practically dead, but I was able to thaw them so gradually and uniformly, that as they relaxed from their rigidity they recommenced to live, and showed, after a short time, no evidence of injury from the temporary death in which they had been held. In these animals the whole of the nervous centres, excepting, perhaps, those immediately connected with the heart, had been brought into inertia by the cold.

Researches with Cold locally applied.

The above was the effect of extreme cold extended to the whole of the vital nervous matter; but another equally singular fact was discovered, namely, that the same effect of cold could be localised, so that parts only of the cerebral or spinal centres could be suspended in function, whilst other parts were unaffected. Thus when in birds the corpora striata were made to sleep by cold, the cerebellum being left unaffected, excited volition pushed the body forward, while, if the cerebellum were rendered insensitive, the body was carried backwards usually with a series of somersaults. These events had their analogies in injuries and diseased conditions of brain in man himself, as in the temporary paralysis of the anterior centres of the brain on looking over a precipice; as in a case where a patient suffering from disease of the corpora striata was impelled to rush forward, careless as to any obstacle or danger that might stand in his way; and as in some conditions of somnambulism, where the impulse to move forward, regardless of consequences, is the dominant impulse; but I dwell now only on the fact that a portion of the brain structure could, it was found, be artificially brought into hybernation by temporary subjection of the affected part to cold, and

that recovery would take place by restoration of the natural condition of fluidity. It was not a little astonishing to find how sharp is the line of demarcation between an affected and an unaffected portion of the cerebral matter. In warm-blooded animals the cerebrum, the cerebellum, part of the medulla, and parts of the cord could be rendered hybernate, and be kept so without danger to life, if the respiratory centre of the medulla were left free. In cold bloods the whole could be affected without destruction of vitality.

Cold under Freezing Point.

It was ascertained by further experimental enquiry that, in order to produce very decisive effects, it was not necessary to carry the cold to the extent of actual freezing of the nervous matter. When cold was applied to a vascular part like the skin there were brought to view three stages of action: (*a*) a stage of exaltation of action in which the part was injected with blood; (*b*) a stage of inertia and insensibility in which the structure was left bloodless, firm, and insensible; (*c*) a stage during recovery, the cold being withdrawn, in which there was return of vascularity with that temporary exaltation called, usually, reaction. In a modified way the same thing occurred when the cerebrum was subjected to cold. The pia mater was at first injected, but its surface is so delicate, it soon was emptied of its blood, and the cerebral substance underneath it was rendered inactive without any extreme reactive condition. For this reason it was found comparatively easy to induce temporary somnolency and sleep by a process of moderate abstraction of heat. In one experiment I determined that drowsiness ending in sleep began when the temperature was merely reduced 6 degrees Fahr. in the nervous mass, and it was this observation that led me to suggest the original theory

that ordinary sleep might well be accounted for as due to nothing more than a molecular change of structure in the nervous organisation owing to the dissipation of energy from the brain and its subordinate parts during long periods of labour.

Physical Modification of Parts under Cold.

In yet another series of researches on nervous matter, both in its dead and in its living forms, I found that under the inertia induced by cold the passage of electrical currents through it was resisted. I made the muscles in this case play the part of the galvanometer. On an animal anæsthetised with chloroform, in the year 1867, the sciatic nerve was exposed, and without dividing it a galvanic current was passed through the exposed portion in the direction of its length. The current was now increased until it caused motion in the muscles supplied by the nerve, attended with a deflection of forty degrees in the galvanometer then used when the current was closed or broken. The nerve was next chilled by the application of ether spray until it was frozen, whereupon the needle fell back to zero, and the muscular motion failed to be excited. Afterwards the observation was varied by freezing the nerve, not between the two points included in the line of the nerve, but at a point above, on the brain side. The muscular motion produced by making and breaking contact was now as completely checked as when the frozen part itself was in the circuit. The facts showed that by changing the molecular condition of nervous structure by cold the same interruption to the course of vibration occurs as when the nervous structure is either firmly compressed or actually divided, but with this essential difference, that the cold produced no more than a temporary suspension of function or sleep, easily recovered from; whilst the

mechanical effects of compression or division were apt to terminate in permanent disability. From still further observation in the same direction, it was found that involuntary muscles, like the diaphragm, could have their work suspended by reduction of the temperature of their supplying nerve in its course towards the centre from which it arises, as well as from the interruption at that centre itself; but that the inertia was most decisive the nearer the interruption was to the muscle, as if the nerve were, in fact, a mere continuation of the centre, and in a minor way were still acting when the centre itself was out of action.

From these experiments I was led to see that electrical vibration was an excellent test of the working condition of nervous matter, but in applying it there was always this obstacle: that in using the continuous current decompositions were apt to be set up which falsified results. Here, therefore, I was stopped for a long time. At last in 1879 I was so fortunate as to become possessed of my friend Professor Hughes' beautiful electric balance as a means of research. Now I had an instrument which measured for me from the secondary coil, and by which I could measure through a scale of two hundred degrees of sound, and establish comparisons from minute variations or conditions of the nervous substance under investigation. I put dead nervous matter, brain or cord, after it had been warmed to its natural temperature in a specially constructed chamber, into the circuit, and noted on the scale the degree of conducting power that was exhibited. I raised the temperature of the structure and tested again; I cooled the structure down to freezing point, testing all the way along at stages of different degrees the conducting power. In the end I obtained results which indicated that the conduction became modified according to variations

of temperature, increasing with rise of temperature and decreasing with fall in steady and distinctive degree. Also, I brought the structures back from the extreme of cold to natural heat and to fever heat with the reverse results, from all which I inferred, I think justly, that no molecular injury was done to them, but that they had passed through the same physical change as the nervous matter of frozen animals in whom there is returnable vitality from what appears to be the absolute inertia of death. The value of the electrical test in this respect could not be overrated; it seemed to me to differentiate between the condition of death and the condition of life as illustrated by the effects of vibrations on animal matter.

INFERENCES LEADING TO NEW OBSERVATION.

From the study of phenomena of research above quoted, I was naturally led to think of the cause of them. It was easy enough to jump at the conclusion that a frozen brain would cease to be an active brain, that a frozen nerve would cease to be an active nerve; it was easy enough after freezing the peripheral surface of nerve, as on the surface of the skin, to say sensibility is destroyed in the part frozen. But this was not satisfactory; it did not explain what the changes effected; it did not account for local manifestations of the particular kind recorded; and it did not explain what parts in the affected nervous structure were modified in character under the process. Here therefore I began to enquire anew.

Giving up, for the time, the theory of a nervous ether, I looked at the construction of the nervous substance from a simple mechanical-physical point of view. In the crude form of it there were the three distinct kinds of matter,

namely ;—water, uncoagulated albumen, phosphorised fat. These parts have their own specific attributes under the influence of heat and cold. The water would not go into solidity under cold until freezing point, and then suddenly with expansion. The fat, however, would pass evenly and by degrees from the fluid into the solid state under cold, and back again by degrees into the fluid state under the influence of heat. The albumen in which the vital endowments would be centred would remain at the normal temperature of the animal body, always fluid, and as free from coagulation as it is in the serum of the blood ; it would be diffused through the water of the nervous substance as it is diffused through the serum ; and of itself it would not be likely to be affected either by the cold or the heat at blood temperature. At the same time it would play an intermediate part as between the water and the fatty substance ; it would cause, as in an emulsion, the fat and the water to unite so as to form a homogeneous compound. If therefore cold were applied to this mixture the inference would be that the whole would undergo gradual cooling, and that the necessary effect of the cooling would be to increase solidification step by step without subjecting the water to congelation. The thought threw new light on construction for function. It suggested to me an explanation of the local action of cold. An expanse or surface of water alone would not show physical change of structure until freezing point was reached, and then a considerable surface would solidify from one point. But what would happen if a surface of a compound of fat, albumen, and water, were exposed to cold ? The experiment was made, and a comparison was struck. A compound as named was exposed to cold, and was found to solidify long before freezing point was approached. Then brain substance, triturated into

solution and freed of membranous substance, was tested in the same manner. It responded in a similar way.

Another observation was made. When cooling of the surface of the compound was localised, the solidification was also local, a fact which corresponded with what had already been observed in the brain in its vital state.

To sum up. The position to which I was led ran as follows. The nervous substance is physically constructed of three parts: *water*, *soluble albumen*, *phosphorised fat*. The water, which in a certain manner is solidified, is susceptible of more complete solidification by cold, with the capability of restoration by warmth. The albumen, soluble in the water, and capable, like all similar colloids of hydration, to any degree, moves with the water in respect to solubility. The fat, rendered soluble with the water and albumen, solidifies under cold more readily than water, and becomes fluid by heat more readily, carrying the water with it in its change of physical quality, by which means the nervous substance under limited range of temperature varies in the consistency of its matter.

THEORY OF WATER POWER AND TENSION.

And now I come to the later development of reasoning from and on research, to which I desire particularly to draw attention. One day it struck me that the action of the refined nervous ether, about which I had worried myself so much, might all be effected by water changing in tension, expanding under elevation of temperature by oxidation, into vapour, and condensing under reduction of temperature. If this were so, then much indeed would be explained. Then we might be led to look upon the brain and its subordinate parts as an independent thermal centre and water power, acting, however

complicate their minute anatomy, as water influenced by the mode of motion called heat.

When the thought was opened there was an immense deal to be said in favour of it. A large part of the nervous matter is water. The degree of physical condensation of water in the closed cavity of the skull is most remarkable. Itself resistant to mechanical compression, it is here, in the simplest way, compressed: compressed into a certain solidity without being frozen, and connected with tubular nerves along which it ought to be able to maintain an extending column of nervous fluid into the remotest parts under the mere impulse of central vibration. If this were so, all impulses would be steadily flowing from the brain into the body during times of cerebral and spinal activity, while all vibrations from the external universe would vibrate back, and unless overwhelming in intensity, would sustain vigorous or subdued action according to temperature, as water in a steam engine in varied tension fashions the motion of the engine.

It was very encouraging to read by the light of theory so much possible natural fact in support of it. It was as if one ignorant of the action of the watch had suddenly found the mainspring, and had felt one's self able from that instant to understand the movement of the hands and the regulation of their courses. Looking now at the central nervous system as a water power enclosed in resistant bounds, so as to be able to exert action under oxidation and the resultant heat, it lay before me as the mainspring of the animal body to which the blood and circulation play an important but, really, subordinate part. To test this view, I passed again to experimental observation, thinking now of water as a motor under various degrees of tension.

I said to myself, If water vapour be the motor under expansion and condensation, it must, after it has served its purpose, be condensed, and this led me to recall an observation which I originally made on the mode of exit of the cerebro-spinal fluid. I found, as detailed in the first number of the ASCLEPIAD in 1884, that the escape of that fluid was by the lower extremity of the spinal cavity into the blood by the inferior cava. From this a fact of supreme importance was discerned. It showed the precaution taken by nature to keep every part of the brain and spinal cord in a water condenser, and the interior of the brain in communion by it with the exterior. The cerebro-spinal fluid is the condensed fluid, and is the regulator of the pressure under the varying moods of oxidation incident to variation of vibration. When the brain and cord are unusually active; when they are receiving vibrations on every side; when, that is to say, the water tension of the centres is at its height, then the amount of condensated cerebro-spinal fluid poured into the veins must be enormous, and may easily and reasonably account for that free action of the kidneys which in hysteria and other kinds of mental excitement is so constantly observed. When, on the other hand, the central tension is reduced, the accumulation of the water in the ventricles and in the arachnoid sac will fill up the void, and by the even pressure it exerts favour quietude and sleep. I considered this to be one of the best evidences of the truth of the theory of the brain as a water power. If the cerebro-spinal fluid should accumulate in excess there would follow, as I have more than once shown, deep stupor, coma. If it were drawn off in excess there would be violent mania. The part played by the fluid is, in fact, in the most refined degree regulatory, and is one of the most beautiful and

important parts of the cerebro-spinal mechanism. It is the dialysed efferent fluid of the nervous centres; it is the condensed fluid of the nervous centres; but, in ebb and flow, according to the tension of those centres, it maintains also equality of balance, and almost certainly governs sleep and wakefulness.

This seemed very natural, and made the theory still more clear as a working theory; but I felt that certain other evidences were wanted, which experiment ought to yield, if the theory were correct. Two views occurred to me in this direction:—

(1) I reasoned that if water plays the part supposed, then removal of it from nervous tissue ought to have the same effects in regard to vibration as condensing it by cold. That is to say, removal of water ought to reduce conduction in proportion to removal, while removal of fat ought to increase it.

(2) If the theory were true there must be in the cerebro-spinal axis a steady combustion or oxidation, that stands alone, that is, in a sense, independent, a sovereignty that works the central power by its own mere motion. It also must be a modified as well as steady combustion, and the products of it must be soluble and dialysable. We ought, therefore, to be able to excite a combustion in nervous matter, even when it is dead.

In order to test the first of these propositions, I followed the same series of enquiries, in regard to electrical vibration after the mere removal of water, as I did when the nervous matter was condensed by cold. Brain, spinal cord, and sections of nerves were subjected to observation under various degrees of hydration. A portion of the substance to be tested was placed in the electric balance, and its conducting power carefully noted. While charged with its water it was placed, at 100° Fahr.,

in the drying chamber in order to be dried down to complete dryness, and as the process went on the conduction was tested day by day. The conduction was found to decline in proportion as the water disappeared until, on complete desiccation, conduction ceased altogether. No demonstration can be clearer. It can be carried, so to say, backwards and forwards. The nervous matter can be dried down to complete inertia, and by exposure to water vapour, which it will readily reabsorb, can be made to resume its full conducting power. The fact explains why in some animals exposed to slow evaporation the nervous system may be brought to such inertia, that death itself seems absolute; and yet, on immersing the animals in water, they revive and relive. This is the equivalent to hybernation from the action of cold.

I made an enquiry relating to removal of fatty matter. It was seen that, under cold, the presence of fat in nervous matter modified the action of cold, so that condensation progressed sufficiently to interrupt function before freezing occurred. The fact led me to ask what would be the effect of removing the fat and letting the water remain. The experiment was tested. Portions of nervous matter, brain and cord, were taken in the fresh state from the sheep just killed, and conduction was tested. The sections were placed in ether or in bisulphide of carbon and left until the fatty matter was dissolved out; then, being removed, the test was reapplied, with the result of finding that the conducting power had increased, by removal of the fat, ten degrees. Sections were tested also in intermediate stages with proportionate increase of conduction, according to the removal of fatty substance.

Hitherto I had made electric vibration the test of activity. I moved to the vibration of sound, and here

again the telephone and balance came to my assistance. By putting nervous material between the vibratory drum of the telephone and the ear, I was enabled to detect variations under many varying conditions of the nervous material. The results were most important. There is not time to refer to them here in detail, and they are too delicate to be made matter of illustration so that all present could verify them at once; but, briefly, they showed that absorption of sound by the nervous matter was most perfect when the water was in full, but not extreme, tension, that removal of tension by condensation lessened absorption, and that high tension increased it.

Turning to the second proposition for enquiry, namely, the combustion that is present in the nervous centres, I was helped considerably by a significant fact which I had learned in my earlier experiments on heat and cold. I had found by direct observation that, whatever was the nature of the combustion going on in the nervous centres, it was attended, in all animals, even in birds—in whose bodies what would be the highest fever heat in men is the natural condition—with a much lower development of temperature, than in other active organs of the body. A difference of 4° Fahr. was observed between the brain and the liver and 2° between the brain and the arterial blood. Moreover, I had learned in the most singular manner that there is going on in the living brain an actual phosphorescent combustion from oxidation, a combustion that would yield a steady low temperature with soluble products that would easily dialyse and make their way out of the nervous substance both by the blood and by the cerebro-spinal fluid. Whether combustion of carbon takes place in the substance of the brain or cord, with liberation of a gaseous product like carbonic anhydride, remains to be de-

termined; but the oxidation of phosphorus there, with production of dialysable products, cannot be doubted. It was necessary, therefore, to take this modified oxidation into account, with the surmise that the lower temperature of the brain in its natural state, compared with that of other vascular organs, is from this cause. On this enquiry I have been long, and am still, engaged.

I have carried out the enquiry by taking the dead brain of the sheep divested of its membranes, and rubbing it into pulp or emulsion. The emulsion is phosphorised by the simple process of mixing with it phosphorus dissolved in carbon bisulphide; the bisulphide is rapidly removed by the air-pump, leaving the phosphorus in the finest state of distribution in the emulsion. The phosphorised brain mass is now experimented on in divers ways. A mould of albuminised tissue involuted into convolutions is charged with the brain substance, and is then floated on richly oxidised blood—derived from the same slaughtered animal—rendered alkaline with soda, and placed at the temperature natural to that blood. The oxidation is sustained for several hours, the blood being often removed and changed for new blood freshly oxidised. Under the warmth the brain pulp oxidises, until a slow combustion is established through the whole structure. It yields, during condensation, a fluid analogous to the cerebro-spinal, a grey exterior, where the blood has dipped, and a white central interior mass.

Into these experiments I introduced another line of research. I mixed with the oxidised blood different foreign substances, in order to see if they were accounted for after the exposure to the combustion. Some, like alcohol, were rapidly changed, others, like strychnine, slowly.

I would like to dwell on these experiments and on

others similar, but my time is nearly exhausted, and I have yet to glance at the bearings of the theory I have advanced on some of those diseases with which you are most familiar. As preparatory to such application let me, however, in a brief summary place before you the argument of the theory under a few distinct heads.

The Argument.

(1) The cerebro-spinal axis is a static thermal centre and a water power—the mainspring of all vital actions. The cerebro-spinal nerves are tubular continuations of the white matter of the centres, producing a fluid responsive to the centres themselves, practically static, and liquid during life, but susceptible, while normal, of receiving and conveying pressures, much in the same manner as the fluid in a barometric tube; susceptible also of rapid condensation on change of condition from the natural state.

(2) In those parts of the centres called grey, where the surface comes in contact with blood charged with oxygen, there is in progress a slow combustion in which phosphorus plays a leading part, maintaining an equably reduced combustion, with the formation of dialysable saline products. The grey matter, the seat of the combustion, takes its colour from the blood, extending to the depth of the blood membrane, dipping into it from its surface, and separated by the convoluted blood membrane into centres, each centre possessing its own surface of oxidation and acting as an independent organ. The white matter, on its part, is the great receptive centre, supplying combustion material, to the grey centres of combustion, as the stem of the candle supplies the wick, but acting also as the receptive medium of vibration to and from the vibrating nerves. In the combustion of the great centres sufficient heat is developed

to bring the whole volume of the centres into proper tension. In the nervous cords the same process is going on, so that under the combustion sustained by the centres the nervous cords, cerebral and spinal, are brought also into natural tension for conveying vibrations from the centres to the peripheries, and from the peripheries back to the centres. The nervous fluid in the nerves is practically static and easily condensed under exposure or injury; but it is most probable that at their peripheral terminations they give up fluid during central pressure, which fluid stimulates muscles into contraction and glands into excretion.

(3) The theory accounts for the grand nervous phenomena of life in their activity during wakefulness, and their repose in sleep. Wakefulness and sleep depend on variations of tension. When the brain is at full, but not too extreme, tension; when the cerebral fire is at full, but not excessive, work, all parts that respond to it are active and wakeful. When the tension is reduced—in other words, when the oxidation wanes—the process of central condensation comes on, with production of cerebro-spinal fluid and phenomena of weariness and sleep, which last until the cerebral fire attains its restoration, tension is restored, and the organic functions subservient to the nervous, including the muscular functions, are brought back to what is called life. The cerebro-spinal system is in fact a true water engine, so true that an artificial engine acting on the same principles could be constructed upon it for the production of motion.

(4) The theory explains the well-known effects of varying external pressures and temperatures on the central nervous organism. Atmospheric pressure tells on it through the nervous expanse, as it does, only in a more refined degree, on the mercury or spirit of the barometer.

Reduced pressure of moderate degree would give a freer expansion to the centres. High temperature without evaporation would produce enfeebled tension; low temperature, in moderate degree, would favour high tension; but an extremely low tension, sufficient to produce actual solidification of nervous surface, central or circumferential, would produce complete cessation of action, a fact that admits of demonstrable proof, local and general, by the action of cold.

(5) The theory attributes to the cerebro-spinal fluid the most important functions. It declares this fluid to be the condensed fluid of the combustion of the cerebro-spinal axis, the regulator of pressure, and the medium by which many poisonous substances are removed from the blood. Charged and recharged with various foreign substances like alcohol, glucose, urea, chloral, strychnine, it eliminates some directly, others, less decomposable, by repetitions of eliminations marked by paroxysmal seizures.

(6) The cerebro-spinal axis is not merely an absorbing centre for the reception of external vibrations, but a true chemical and dialysing centre, and the centre of the static combustion, by which under the fluid pressure, regulated by the spinal fluid, the nervous tension is sustained in all parts of the body having nervous communications with it. It is a true physical autonomy.

(7) Under this theory, ganglia are supplementary centres, supplied from the main source. They lie as intermediates between the great centres and the involuntary muscles, feeding the involuntary muscles with nervous stimulus in steady and continuous supply so long as they are steadily supplied themselves, but exciting the muscles, when oversupplied, to overaction. Plexuses, in their

turn, are intercommunicating points or meetings of nerves in order to enable vibrations to be carried on should one or more nerves belonging to a plexus fail in function from disease or injury. Decussating fibres are explained as the means by which centres are prevented from becoming independent of each other and losing compensating balance.

(8) If the theory be correct, two distinct combustions exist in the animal body, one the central or nervous combustion, a combustion leading to low tension and pressure, the static combustion; the other that higher combustion of the muscular and active vital organs, the combustion yielding the animal heat which we recognise as the sensible heat of the body, having its own independent and, apparently, more active function; and yet, possibly, dependent for its continuous existence on the slower and central combustion which keeps it alight, and regulates its activity by regulating its supply of blood and, therewith, its oxygen and its combustible material.

PRACTICAL APPLICATIONS OF THE THEORY TO SOME FORMS OF CEREBRO-SPINAL DISEASE.

Under this theory an immense number of explanations of phenomena hitherto unexplained come, I hope, into view with perspicuity. Whatever should quicken or exalt the nervous combustion at the centre should quicken the current of impulse along the nervous tracts and excite muscular motion from action to overaction. Whatever should reduce the central oxidation should reduce the nervous currents, and produce sleep, or in extreme degree collapse and inertia, or death. Let there be removed, rapidly, the supply of oxygen carried by the blood, let the brain fire, that is to say, be suddenly put out, as in

hæmorrhage, and so rapid will be the condensation that for a brief interval, under the pressure, the nervous influx into the muscles will throw them into convulsion and rigidity. Let the brain temperature be raised as in fever, and straightway there must be an excessive nervous excitement, an overflow of nervous current, with quickened action of the involuntary muscular pulsations and direct radiation of the increased heat from the cutaneous surface of the body, the mucous surface of the viscera, and the serous expanses. What is called a chill from exposure of the body to cold or wet is explainable on this theory. The peripheral nervous surface, arrested in radiation, receives a check primarily, followed by a necessary excess of temperature and that fever which always follows sudden arrest of peripheral function.

The phenomena of inflammation, local and general, are also explainable on the theory without any complication or difficulty; but what I would now notice more particularly is the exposition it offers in relation to some forms of cerebral disease.

Reflex Action—Epilepsy.

'The phenomenon of reflex nervous action is by it rendered explicable. The impression on the peripheral surface, which gives rise to the reflex movement, after vibrating along the aqueous line up to the centre, radiates out at the centre, and exciting quicker oxidation there, causes an impulse which produces local central injury with return vibration back by the nerves to be dissipated in the muscle or muscles which the conducting nerves supply. But for this time is required, and therefore the time of the reflex. If the impression be too severe and too universal there may be no reflex, but an actual injury to the nervous centre, a stun, a stroke, or an apoplexy, fatal possibly, or

if not followed by a reactive flash of vibrations from the centre to great groups of muscles, causing general convulsion. Epileptic seizures, on this argument, may be peripheral in origin, due to an intense vibration sent to the grand centres, temporary increase of vibration there, and radiation back into the muscles until, by exhaustion of the excitement by the muscles, equilibrium is restored. In other words, if there were no epilepsy there would be apoplexy and death, so that the very phenomena of the seizure are indications of the mode by which its occurrence and repetition are compatible with continuance of life.

Mania.

By this same theory acute mania is logically explainable as a fever; an overaction of the great nervous centres, springing up originally either in them, or from quickened oxidation developed in some part of the nervous expanse, in periphery, as in acute pneumonia or pleurisy.

Mania may also be accounted for from changes in the cerebro-spinal fluid. If the cerebro-spinal fluid were rapidly drawn off, the inevitable result must be convulsive movement and spasmodic movement, with intense excitement, up to tetanus. If it were drawn off slowly with other fluid from the blood, the result would be collapse with spasms, as in cholera. If it accumulate in quantity, the result would be coma from pressure with convulsive movements. If it collect into itself toxic substances, the result will vary according to the substances.

Structural Changes.

The theory applies to the explanation of structural changes in the nervous masses themselves. Excess of fatty matter in them must reduce the oxidation. Increase

of water in the substance must also lessen oxidation, induce pressure, lead to general absence of tension, and cause paralysis. Alcohol in the centres must lead to quick and temporary expansion and excitement, probably from combustion of it there, followed by extreme condensation, stupor, and exhaustion. Many times repeated, the action of alcohol in producing general palsy is a necessity in those who are unable to eliminate the substance with rapidity.

Toxic Substances.

One more word. I found in experiments on cerebral oxidation that the process was very much impeded by the presence of some foreign substance. If, for instance, the carbon bisulphide were not well removed, the oxidation was checked. It has been observed that workers exposed long to the vapours of carbon bisulphide become affected with a special paralysis and cerebral failure, and now we see that this phenomenon is, under the circumstances, inevitable. I name this because similar interruptions to cerebral oxidation may be induced by other disturbing substances, and melancholia and hypochondriasis may be traceable, ultimately, to some persistent disturbance of this nature.

By this theory the phenomenon of alcoholic craving is naturally accounted for. If alcohol is burned in the brain-fire and feeds it, the crave for alcohol may well be as insatiable as we know it to be in the alcohol-stricken.

COTEMPORARY PRACTICE AND LITERATURE.

"Every physician will and ought to make observations from his own experience; but he will be able to make a better judgment and juster observations by comparing what he reads and what he sees together."

"DIPHTHERIA: ITS NATURAL HISTORY AND PREVENTION."

[BY R. THORNE THORNE, M.D., F.R.S.]

THIS volume (Macmillan and Co.), by the learned Assistant Medical Officer of the Local Government Board, contains the "Milroy Lectures," delivered before the Royal College of Physicians, in the course of the current year. There is no preface in the technical sense of the word, but the first chapter may be considered as a fine prefatory introduction, full as an egg of information, and regrettably serious in the knowledge it conveys. It tells the fact, which many of us have been slowly learning, that, whilst other so-called zymotic diseases have been declining under our advancing sanitary improvements, diphtheria has been on the increase, and it tells the still more serious fact that, with all our labour of research, those who have laboured most in investigating the causes of the disease are far away from commanding knowledge in regard to it. How nicely and modestly the author puts this information before his readers is shown on page 10, a page composed with infinite care, and with, I doubt not, a foreknowledge that if it should

get the chance of being largely read, it must arrest an almost universal attention. He excuses himself for appearing as an author on diphtheria by remarking that, in the midst of all our insufficiency to grapple with the disease, we have reached a position in which there would be an advantage in taking stock of such knowledge as we have acquired in respect to causation and prevention; "and whilst tracing the stages by which such knowledge has been obtained, to emphasise those points which stand out prominently as affording an indication of the lines of research which if followed out may be expected to clear up some of the obscurity in which the etiology of diphtheria is still involved."

Dr. Thorne thinks that diphtheria has long been present in our country, although not probably recognised. I quite agree with him on this point, and could, if I had time to look them up, find some additional proofs in this direction. Thus I remember some thirty years ago dropping upon a letter addressed by one "J. F." to Dr. Heberden, in which, under the head of "Malignant Sore Throat in 1758," the writer described what could be no other disease. It was ushered in with all the usual symptoms of diphtheria as we now know them; it was very virulent, and the dusky red tonsils presented soon a grey slough looking like the worst form of foul venereal ulcer. "The disease," this observer said, "had only been noticed by two previous authorities, had been buried a hundred years, and would have escaped attention in his time had it not led to a calamitous incident, namely, to a death in a family of great character and distinction." Dr. Thorne supplies much similar evidence, and the worth of it is invaluable, since it breaks through the common and misleading idea that the disease was brought over originally from Boulogne in our own times, instead of

having been a lurking disease on our own soil for many centuries past.

On the subject of the nature of the disease the author looks upon the affection as distinctive, and suggests that when it has been connected with other affections, such as scarlet fever or croup, the connection was one of coincidence rather than of interrelationship. He considers it a local affection proceeding from the local to the general, and that the fauces form the point, or local spot, in which the surface and typical lesion, the lesion that gives origin to the future evils, finds its primary centre. It is difficult to dispute these suggestions; and on the unity of the disease it is impossible to differ. The only difficulty is why one particular part should be the particular seat, at first, of the cause that excites the constitutional mischief. In what respect does this particular patch of mucous membrane differ from the continuous lining of the cheek, or the lining of the nasal canal? True, in glanders we see a similar phenomenon of selection, the nostril being in this case the part of mucous tract selected, whilst the fauces are left free. But in either case why?

Of necessity Dr. Thorne discusses at great length and fulness what is commonly called the poison of diphtheria. On page 196 he expresses the view that the local action of a bacillus that has been found in the diphtheritic membrane is essentially different from the constitutional effects produced by a chemical product developed by the bacillus. But on the following page he adds, "it is by no means clear what the poison consists in," the expression of a reasoner half convinced and yet wanting to be convinced; of one who sets coincidence at a respectful distance, his eye not actually off it, but askance, with a wise and far-seeing prudence. It had been well,

perhaps, if he had done the same in sections 15, 16, and 17 of his conclusions, in which a change or two of verbs would have been, at least for a time, a marked improvement; the absolute in scientific argument being ever dangerous until the proofs are absolute; with which thought I close a volume I have read from Alpha to Omega with profit, gratitude, and admiration.

QUAIN'S "ELEMENTS OF ANATOMY."

[EDITED BY E. A. SCHÄFFER, F.R.S., AND G. D. THANE.]

THE volume in hand is the second part of Volume I. of Quain's "Elements" (Longmans, Green and Co.), and deals with General Anatomy or Histology, the whole of the part being edited by Professor Schäffer. It is a volume of 431 pages, together with a good working index. It is splendidly illustrated with 500 engravings, and at the end of each section the learned editor has supplied a rich list of references from all reliable workers and authorities, so that the possessor of this volume, be he a student ever so young or ever so old, has the opportunity of knowing where to go whenever he wishes to find a fountain-head, and of becoming as deeply learned in the literature of his subject as his master. The labour that has been bestowed on the work is remarkable, and the expense to which the enterprising publishers have gone for perfect illustrations calls for the highest praise. To say that the work is re-edited (this is the tenth edition) by Professor Schäffer is not really fair to him. By the new arrangements he has introduced, by the new method as well as matter now rendered, and by the simplicity as well as the solidity of the labour, the editor shows himself as the author of a new work founded

on an old one. This means a laborious task, not always grateful, and very often unsuccessful; a task like transforming a building—because of its antiquity and celebrity—that had better have been pulled down and rebuilt. In the present instance the success is complete; for that which is good in the old is worked into the new so well, the whole stands firmly and in harmony. The book is written for young students, and is priced to meet their means—12*s.* 6*d.*—but one can foresee numbers of old students who, in the midst of active practice, will spend many a stolen hour burning the student's lamp over the microscope and Schäffer.

THE ANNUAL OF THE UNIVERSAL MEDICAL SCIENCES.

[EDITED BY CHARLES E. SAJOUS, M.D.]

THE fourth series of this Herculean work, by Dr. Sajous, assisted by seventy associate editors and over two hundred corresponding collaborateurs and correspondents, has now been delivered to the reading world by its enterprising publisher (F. A. Davis, of Philadelphia, New York, Chicago, Atlanta, and London). The work might be called the medical Septuagint. It is a marvel of labour, and as honest a labour as ever was brought into light. When the five annual volumes stand on the shelf of his library, their owner may safely feel that the substance of every medical essay of the past year, of a practical kind, is at his command. The editors and collaborateurs vie with each other in producing the best record. The principle acted on is that each writer shall give analysis only with as little critical observation as possible. The volumes come before us, therefore, as a kind of progressing cyclopædia.

Did I say progressing? Yes, and that is the word if

advancement be not understood by it too literally. The volumes are full of movement; they are living pages; but, unhappily, they do not record much advancement, and, although earnest readers will always keep them under hand, and use them as reference, few, very few, will read them through from beginning to end for the love of reading them. They in themselves, in fact, indicate no love of reading amongst the profession at large, but much love of writing without reading. This is the taste of the day; all write, few read. What is read is current and casual, so that literature a few months old is considered stale, flat, and unprofitable. As for reading from the far past, that is quite out of the question. If it were not, these five volumes of the immediate past had never been born. The amount of mere repetition in different, and too often in indifferent, tone and manner is incredible, and shows a waste of intellectual labour that is truly deplorable. All this will go on until fundamental truths are arrived at—arrived at, not by study of laborious reproductions such as these, but by simple research of a pure and independent character direct from the book of nature, which is, after all, easier to read and much more satisfactory to study. Nevertheless, Dr. Sajous and his friends deserve the heartiest thanks for a devoted industry that will live when nearly all that it has been expended on has passed into oblivion. A century to come these volumes will have a special value, not so much from the real worth of the details they supply—for all that is worthy of immortality will live by itself—but as indicating the medical mind and taste of a closing century, the "*flux de plume*" of a period that stirs the thousands into the ambition of authorship, makes every man a law unto himself, and renders stamped individuality a golden coin of the past.

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[BY RICHARD NEALE, M.D.]

DR. NEALE'S "Digest" has already been noticed in these pages with all the commendation such a useful and practical work demands. Steadily this book has won its way until it has reached a third edition, a success for which there need be no surprise. Far less ambitious in design than the work of Dr. Sajous above noticed, it is, in a certain sense and within its own limitations, as useful, for it supplies heads without tails and is a digest without quotation. In it the general outline of any particular subject is gathered from a few rapid glances, and the references are so practical and so well arranged that the book, because it is not only an index, but a digest, becomes more useful than the indexes of the journals and works to which it directs attention.

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November 15th, 1891.

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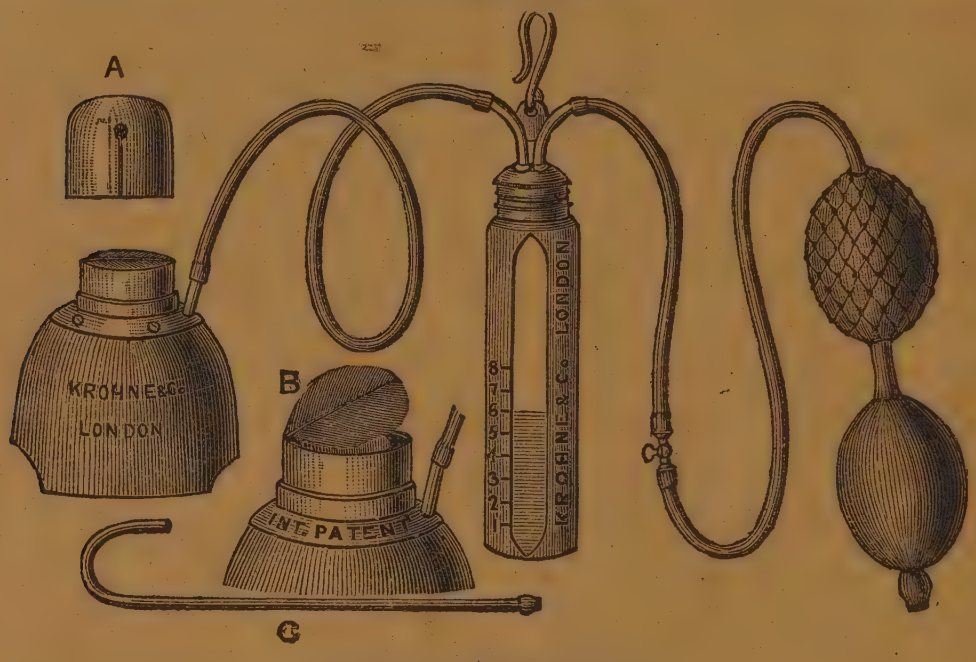

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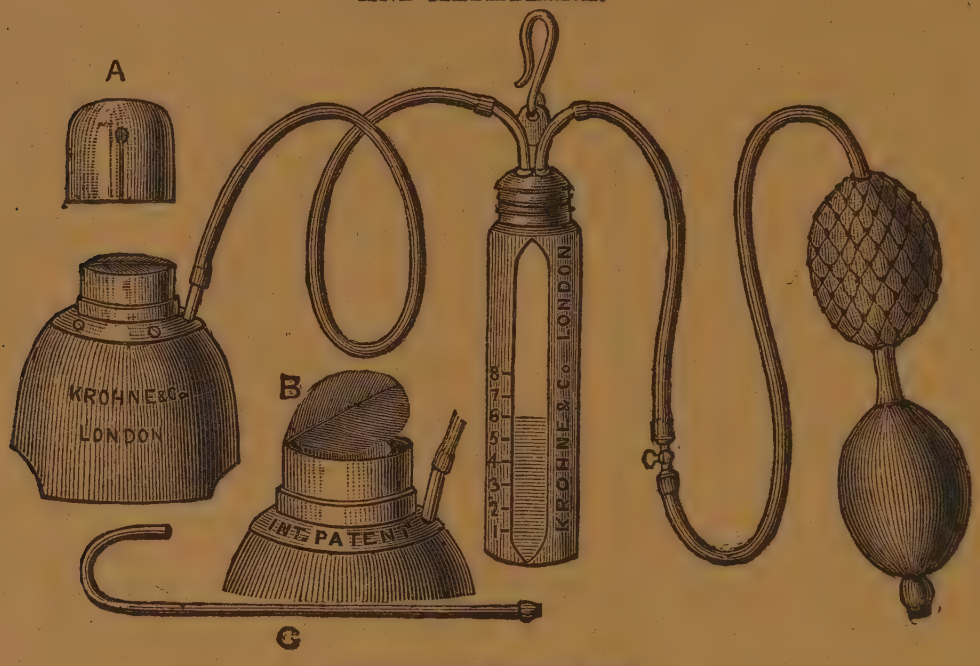

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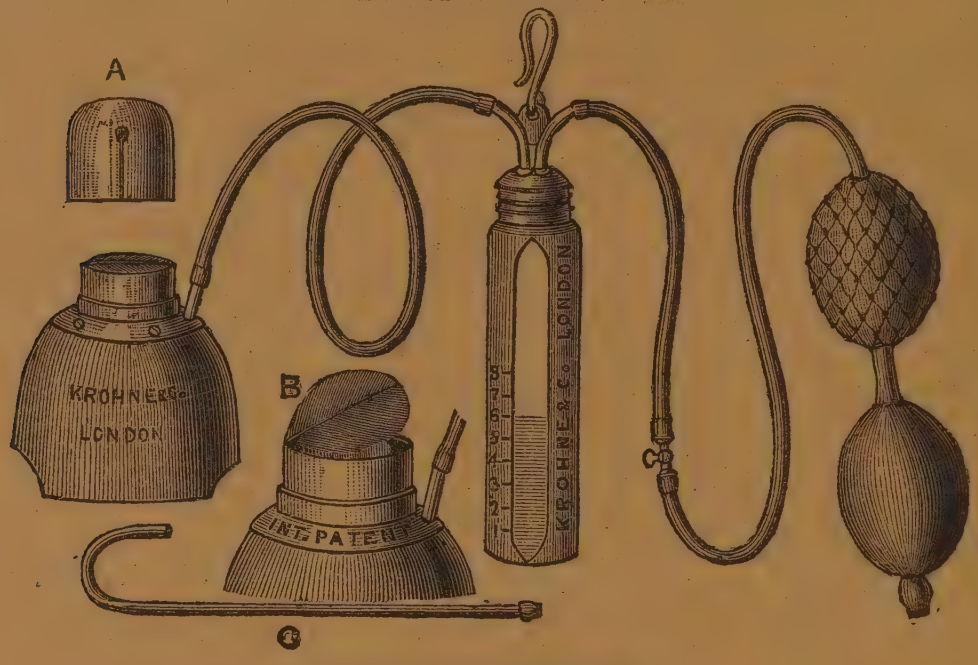

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BY

Benjamin Ward Richardson, M.D., F.R.S.

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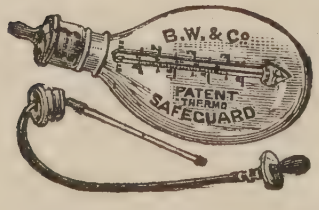

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